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PROCEEDINGS

OF

THE PHYSICAL SOCIETY

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October 1897.

I. *A Naval Range-Finder.*

By Prof. W. Stroud, *D.Sc., M.A.**

Having been engaged for the past eight years in conjunction with Prof. Barr in the construction and perfecting of naval range-finders, it was natural that I should turn my attention to the possible application of telemetrical principles to physical measurement.

One very obvious application is to the determination of (1) the radius of curvature of a concave and especially of a convex surface, and (2) the focal length of a convex and especially of a concave lens. The problem of finding the position of the image of an object in a convex mirror or a concave lens is essentially a range-finding problem.

For the particular purpose in view the most suitable form of range-finder is a constant-range instrument, *i. e.* an instrument (whose optical parts are rigidly fixed) which can be translated along an optical bank and will indicate when an object or image is at the specified range. The mode of action of the instrument will thus be very like that of an ordinary short-focus telescope as commonly used on optical banks, but

* Read October 29, 1897.

a short-focus telescope is essentially bad as a range-finder, and that for the following reasons :—

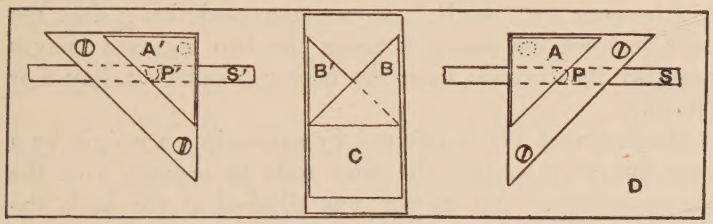
(1) As is well known, when using such a telescope it is advisable to cover up the middle portion of the objective so that only the extreme beams are used to form the image. In this way a crude range-finder is formed, which suffers from a radical defect, arising from the fact that the beams enter the eye in different directions, so that any alteration in the accommodation of the eye produces a duplication of the image. An attempt is made to avoid error from this cause by keeping the eye focussed all the time on cross-wires fixed in the focal plane of the eyepiece. (2) The short-focus telescope has not usually *base* enough for accurate telemetry (the maximum available base being the diameter of the objective), or if it should have base enough, the minimum range which it can measure is too great.

Looked at from the range-finding point of view the problem of optical spherometry and focometry is one of excessive simplicity. What is required is a range-finder whose prime reflectors can be fixed at any distance apart (to furnish the adjustable base) and at any angle to that base (to furnish the adjustable range).

One arrangement is shown in plan in fig. 1 and in perspective in fig. 2. A, A′ are two triangular reflecting prisms forming the prime reflectors supported on stands provided with levelling-screws. Slots S, S′ are provided in the frame-piece D through which pass stout pins P, P′ provided with clamping-screws below. B, B′ are fixed reflectors opposite A and A′ respectively. C is an extra reflector (made into one block with B and B′) so as to reflect the two beams upward to the eye. If now the prime reflectors be adjusted for a particular range, the two portions of the object viewed will appear in coincidence in the field of view furnished by C. If the two portions are not in coincidence they may be brought into coincidence by translating the range-finder to or from the object or image. The instrument works in fact just like a short-focus telescope, the only difference being that the correctness of the range is determined by observing whether or not the two portions of the object viewed are in alignment.

The instrument was used to determine the radius of curvature of a convex mirror by the ordinary method. It is clear that u should be made as large as possible so as to make v large.

Fig. 1.



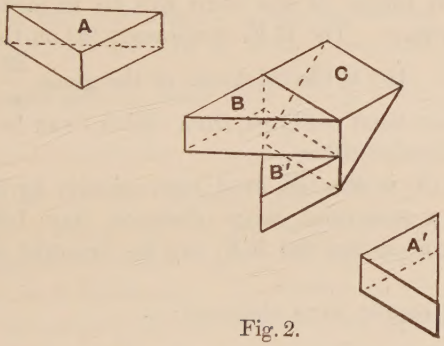


Fig. 2.

The object (a thin rod or slit well illuminated at the back) was placed at the extremity of the bank, and the mirror at the other extremity ; the R.F. (range-finder) was brought up to read (1) the image of the rod, (2) the surface of the mirror, (3) the position of the object. Measurement (1) is first done because the R.F. should be adjusted on the image so as to have maximum available base-length and minimum range (the conditions for maximum accuracy). The portion of the mirror used was restricted to 10 cm. diameter, so as to make the measurements comparable with direct measurements made with our spherometer, which has its adjustable point 5 cm. distant from each of the fixed legs. The R.F. is then brought up to within a few cms. of the mirror and the prime reflectors are angled until the images appear nearly in coincidence, the prime reflectors are then rigidly fixed and

the R.F. is moved to and fro along the bank until exact coincidence has been obtained, when the reading on the bank is taken. If the base-length is 10 cm. and the image about 70 cm. away, the extreme variations cannot amount to 1 mm. and should not amount to more than $\frac{1}{2}$ mm. From fig. 1 it will be seen that the R.F. is so constructed that it does not itself obstruct the beams forming the two separate images provided its distance from the mirror is sufficient (say 8 or 10 cm.).

Measurement (2) is effected by suspending a weight by a very fine wire so that the wire rests in contact with the convex surface. Our mirror was silvered at the back, the wire resting against the front glass surface. Under these circumstances an image of the wire will be formed by reflexion in the surface. The R.F. is adjusted (*a*) on the wire, (*b*) on its image. If t is the thickness of the glass, $\frac{2t}{\mu}$ is the difference between these readings, from which t can be calculated with sufficient accuracy.

Measurement (3) is effected most conveniently by shifting the rod or slit a measured large distance (say 100 cm.) towards the mirror, so that the R.F. can be brought to bear upon it.

The following results were obtained :—

u.	v.	r.	r (corrected).
143·62	26·82	65·50	65·37
128·79	26·09	65·44	65 31
118·78	25·66	65·46	65·33
124·12	25·88	65·39	65·27
119·12	25·67	65·44	65·31

The last column gives the value for the radius corrected for aberration. The radius of the same mirror measured by our spherometer gave 65·31 and 65·37 cm. It should be remarked (1) that our spherometer has received a good deal of rough usage, (2) that the spherometer measures the curvature

of the external surface, while the telemeter was used to measure the curvature of the silvered surface. Still the results show that this telemetrical method is nearly as accurate as direct spherometry.

The constants of a concave lens were next determined. One face was obviously concave, the other looked nearly flat. The radius of the concave face was first got in this way :— the rod, suitably and strongly illuminated, was placed between the R.F. and the concave surface, and roughly adjusted so that it and its image were in close proximity near the centre of curvature. In other words u and v were made nearly equal and the value of each determined. In this case we are dealing with a real image and can bring our R.F. as near as we please to the image.

Now a little elementary calculation shows that if R be the range, dR the error in R, B the base, and $d\theta$ the error in angular estimation, then $d\theta = \frac{d\mathrm{R}}{\mathrm{R}} \cdot \frac{\mathrm{B}}{\mathrm{R}}$; but $\frac{\mathrm{B}}{\mathrm{R}}$ is practically fixed, as its maximum value $= \frac{\delta}{\rho}$, where δ, ρ are the diameter and radius of the surface respectively. Hence as $d\theta$ is fixed ($\doteqdot 1$ minute of arc for snapshot naked-eye work and $\doteqdot \frac{1}{3}$ minute when great care is taken), dR will be a minimum when R is a minimum. To determine the range of a real image as accurately as possible, we must then take the minimum base and minimum range for maximum accuracy. In this case it may be advisable to place on the face of the prism a convex lens whose focal length is equal to the range required.

In the case of the lens used $\delta = 8$ cm. $\rho \doteqdot 16$ cm.; so that

$$d\mathrm{R} = \mathrm{R} \cdot \frac{\mathrm{R}}{\mathrm{B}} \cdot d\theta = 2\mathrm{R}\, d\theta.$$

If, then, $d\theta = \frac{1}{3}$ minute $\doteqdot \frac{1}{10{,}000}$ and R = 5 cm., then

$$d\mathrm{R} = \tfrac{1}{100} \text{ mm.}$$

In order to compete in accuracy with this a spherometer would require to indicate to $\frac{1}{3000}$ mm. In short, as a R.F. could be constructed to work at much shorter ranges than even 5 cm., there can be no question that for concave spherical surfaces the telemetrical method is more accurate than the spherometer.

In this way (though not with a very restricted base, for I did not realize at the time that the accuracy would be much increased by restricting the base and diminishing the range) the radius of curvature was found to be 15·81. A small spherometer gave 15·87 cm.—the lens face was too small for the large spherometer previously mentioned; the radius of the circle of contact of the small spherometer was only 1·5 cm., and as the instrument was home-made and only ordinary care had been bestowed on cutting the screw, I attribute the discrepancy to errors in the spherometer.

The second face when measured telemetrically turned out to be very slightly concave, the mean of several measurements giving a radius of 3300 cm.

The thickness of the lens can be readily got on the optical bank in this way:—The R.F. stand is so constructed that it can be turned through 180° about a vertical axis. A fine wire carrying a weight at its lower end is supported about the middle of the bank and readings of its position are taken with the R.F. from both sides.

The wire is now replaced by the lens and the operation repeated with the R.F., the readings for the one face being taken from the one side, the readings for the second face from the other.

The differences between the sets of readings for the wire and the corresponding sets for the lens gives the thickness required.

The principal points, the focal length, and the refractive index can now be obtained in the ordinary way.

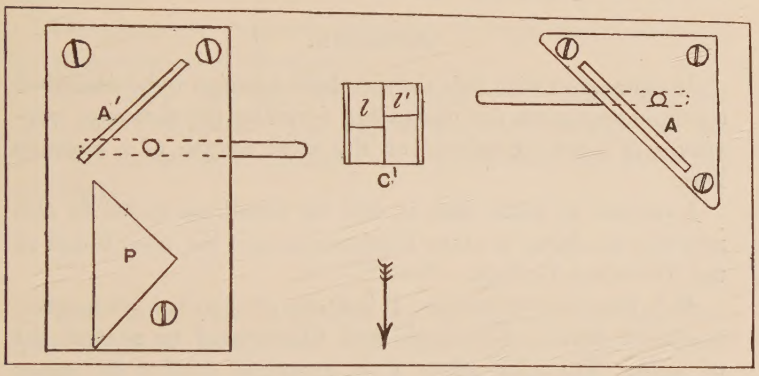
The following results were obtained for the focal length for sodium light, u and v being referred to the principal points.

u.	v.	f.	μ.
24·10	13·33	29·82	1·5276
31·40	15·31	29·87	1·5268
32·13	15·45	29·75	1·5287
35·21	16·13	29·77	1·5284
	Mean	29·80	1·5279

The results are remarkably good when it is remembered that all the observations are simple naked-eye observations without the assistance of any magnifying-power.

Fig. 3 shows in plan another form which the R.F. may take. A, A′ are, as before, the prime reflectors. The composite block of prisms B, B′, and C (figs. 1 and 2) is now replaced by a single right-angled polished speculum-prism C′ turned edge upwards, as shown in the plan. As it stands, this instrument would not be a range-finder at all, but an

Fig. 3.



azimuth indicator. A right-angled glass prism P is, however, mounted on the stage supporting the reflector A′, so that the beam of light is reversed right for left before falling on A′. This converts the instrument into a range-finder. The kind of appearance that is presented in the instrument before correct alignment has been attained is shown by the lines l, l', as seen in the prism C′. Here the object being observed is a line perpendicular to the plane of the figure at some distance away in the direction of the arrow. A moment's reflection will show that the R.F. is too near the object. To produce alignment we should require to move the instrument in the opposite direction to that of the arrow. The advantage which this arrangement presents over the preceding is that the speculum-prism C′ obstructs very little of the beams. Experiment shows that the accuracy obtainable with it is not appreciably different from that obtainable with the instrument first described.

Conclusions.

1. The telemetrical method described is not much inferior to a spherometer for measuring the radii of curvature of convex and approximately plane surfaces.

2. The method is superior in accuracy (though not of course in convenience) to the spherometer for measuring the radii of curvature of concave surfaces.

3. The method is available for determining all the geometrical or optical constants of either a convex or a concave lens on the optical bank alone.

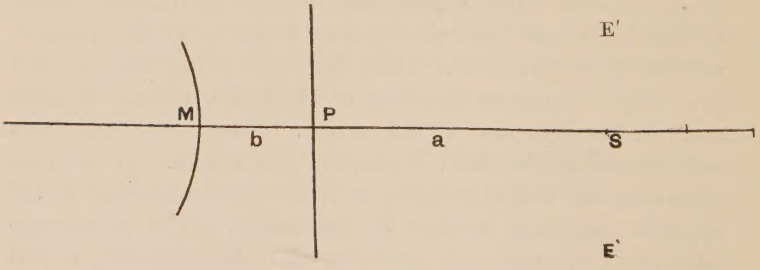
Appendix.

In connexion with this subject there remains to be described a simple apparatus for measuring optically the radius of curvature of a convex mirror or the focal length of a concave lens.

I venture to think that it will be found as useful in elementary teaching in other laboratories as it has been found at the Yorkshire College.

M is the convex mirror, P is a slip of silvered plate-glass (assumed plane), S is a pin well illuminated or narrow slit of light. The plate-glass P is arranged so that its upper edge is nearly on the horizontal axis of the mirror. Either S or P is shifted nearer to or further from M, until the image of S in P coincides with the image of S in M, whether viewed

Fig. 4.



from E or from E′. Under these circumstances $u=a+b$, $v=a-b$, whence r can be obtained. As the image formed by reflexion in M is smaller than that formed in P, it is advisable to use a very narrow slit at S.

To determine the focal length of a concave lens, the mirror is removed, the concave lens is placed so that its face touches the silvered back of P, and is half covered by it. A stout vertical rod is placed at some considerable distance behind the mirror on the prolongation of the axis of the lens, and the pin S is shifted to or from the mirror until the image of the pin seen by reflexion in the plane mirror coincides with the image of the rod as seen directly through the lens, whether we look from E or from E′. This gives the position of the virtual image of the rod, whence the focal length can be calculated.

The pin or rod can with advantage be replaced by a vertical straight edge well illuminated.

Both these methods will be found very satisfactory for elementary students. The apparatus is very simple, and the method seems to me instructive in illustrating the subject of parallax.

Discussion.

Mr. Barr drew attention to the gimbal arrangement and the three struts that keep the supporting rod central in the tube. To give some idea of the precision and scope of the range-finder, he observed that they were there using the equivalent of a 25-ft. "circle," and their measurements were comparable to the measurement of 20 secs. of angle, on such a circle. The instrument is handled by ordinary seamen, and stands rough usage on board ship for years without injury.

II. *Exhibition of a Focometer and Spherometer.* *By* Prof. Stroud.*

[Abstract.]

Prof. Stroud explained that in determining curvatures and focal lengths some telemetric method was necessary, and that, owing to want of parallelism of the beam and duplication of images, a short-focus telescope was always an inefficient telemeter. For the measurement of inaccessible lengths it was therefore better to use some simple form of "range-finder." Such an apparatus could be made with a set of small mirrors, arranged in such a manner as to direct two images of the distant object into an eyepiece, with a fixed prism in the path of one of the incident beams. By sliding this instrument along the optical bench, one position could always be found at which the two images, as seen through the eyepiece, were in coincidence. He also described a method for determining curvature by interposing a plate of plane glass between the curved mirror and a source of light.

III. *Experimental Exhibitions.* *By* Mr. Ackermann.*

[Abstract.]

Mr. Ackermann exhibited two experiments:—(1) The blowing-out of a candle-flame by the air from a deflating soap-bubble. The bubble was blown at the mouth of an inverted beaker by breathing into a hole cut out at the top. This hole was then presented to the flame, and the flame was immediately quenched. But if the bubble was blown from ordinary air with bellows, the flame was merely deflected without being extinguished. (2) It was shown that a miniature boat, provided with a false stern, consisting of a linen diaphragm, could be propelled by filling the hollow stern-space with ether, or with some liquid similarly miscible with water. The motion is due to the continuous release of surface-tension behind the boat.

* October 29, 1897.

Prof. Boys said that when he tried, some years ago, to blow out a candle with a soap-bubble filled with common air, he found the operation very difficult—so difficult that, having once succeeded, he never repeated the attempt. It had not occurred to him, as it had to Mr. Ackermann, that the CO_2 present in the breath played a part in the quenching. With regard to the second experiment, he had seen a small boat propelled by dissolving camphor astern, but he thought the use of a liquid for that purpose was a novelty.

IV. *On the Isothermals of Ether.*

By J. Rose-Innes, *M.A.*, *B.Sc.**

In a paper "On the Isothermals of Isopentane," read before the Physical Society last May†, I gave an account of some results obtained by investigating Prof. Young's experimental work on Isopentane. The main conclusions arrived at were as follows:—

(i.) The study of a diagram constructed by plotting $\frac{1}{av^2}$ against $v^{-\frac{1}{3}}$, where a is the internal pressure and v is the volume of a gram, suggests that there is a discontinuity in the slope of $\frac{1}{av^2}$. Even if there is not discontinuity in the exact sense of the word, there is an extremely rapid change of behaviour, amounting practically to the same thing.

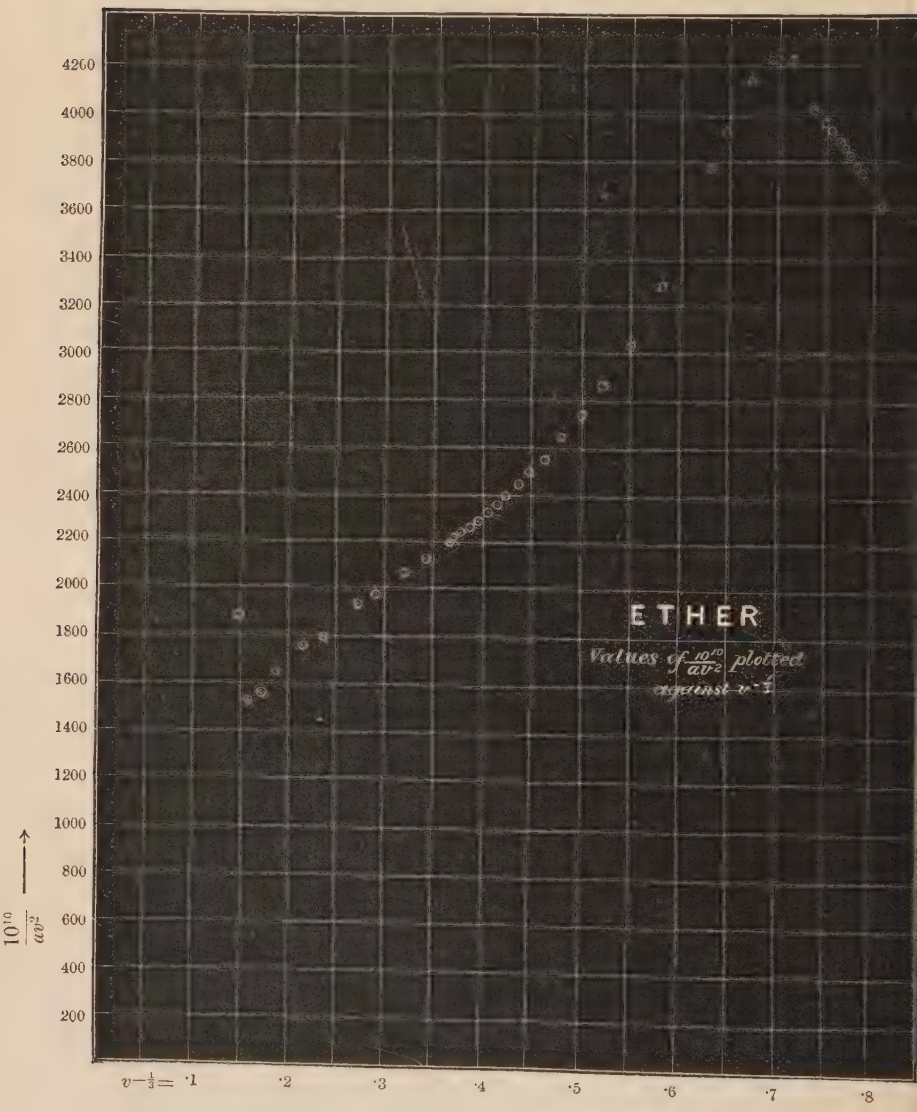
(ii.) The temperature for which the pressure is accurately given by the laws of a perfect gas at a given volume, remains practically constant for all large volumes, until we approach the neighbourhood of the critical volume. At the critical volume this temperature has diminished somewhat from its value for large volumes, but the diminution is only slight.

These conclusions were embodied in a formula giving the pressure in terms of the temperature and volume, and a comparison between calculation and experiment was effected by means of a diagram. It is not suggested that this formula

* Read November 12, 1897.

† Proc. Phys. Soc. xv. p. 126.

is incapable of further improvement, but the close correspondence between calculation and experiment seems to show that



the main conclusions on which the formula was founded are correct.

A natural extension of the above investigation is to try how far these general conclusions are true of some other substance. For this purpose I resolved to employ the experimental results obtained by Profs. Ramsay and Young with ether, as it appears that the linear law connecting temperature and pressure at constant volume holds accurately in this case. [Phil. Trans. vol. 178 A, pp. 57–93; and Phil. Mag. vol. xxiii. pp. 435–458.]

Profs. Ramsay and Young have given the values of a for a large number of volumes (Phil. Mag. vol. xxiii. p. 441), so that it was easy to calculate the values of $\frac{1}{av^2}$, and to plot them against $v^{-\frac{1}{3}}$. The resulting diagram is given on p. 12, and it suggests very strongly that there is a discontinuity of slope at about vol. 3·3. In this respect, accordingly, the ether results show a striking likeness to those of isopentane, the discontinuity in the case of the last substance occurring at about the same volume.

It was shown in my former paper that when Ramsay and Young's linear law holds, there is one and only one temperature for each volume at which the pressure is given by the laws of a perfect gas; if we call this temperature τ we easily have

$$\tau = \frac{av}{bv - \mathrm{R}}.$$

The values of b and a are given by Profs. Ramsay and Young (*loc. cit.* p. 441); the values of τ were thence obtained, and the results are given in the following table:—

Volume.	τ.	Volume.	τ.	Volume.	τ.
300.....	1035	17	792·2	4	815·3
250.....	860·6	16	795·2	3·7	813·7
200.....	827·3	15	795·5	3·3	796·7
150.....	841·1	14	796·7	3·0	769·7
100.....	797·5	13	798·0	2·75	735·5
75.....	766·9	12	800·7	2·5	681·9
50.....	783·8	11	801·6	2·4	662·6
40.....	774·3	10	802·9	2·3	643·2
30.....	775·3	9	805·5	2·2	621·5
25...	780·2	8	805·0	2·1	597·2
20.....	790·1	7	805·6	2·0	571·2
19.....	789·2	6	805·5		
18.....	789·4	5	807·2		

In calculating this table, the value of R was taken $= \frac{1}{\cdot 00119}$.

An examination of the table shows that τ remains nearly constant for all large volumes down to about vol. 3; its numerical value throughout this range is roughly 800. The only exception occurs in the case of vol. 300, but the value of τ is here so erratic that it is clearly subject to a large experimental error.

We have still to try how far the formula found for isopentane can be made by alterations of the constants to suit the experimental results with ether. For isopentane it was found that fairly good concordance with experiment could be secured by the use of the formula

$$p = \frac{RT}{v}\left\{1 + \frac{e}{v + k - gv^{-2}}\right\} - \frac{l}{v(v+k)},$$

where R, e, k, g, and l are constants characteristic of the gas*. I have calculated suitable numerical values for these constants in the case of ether from Ramsay and Young's original experimental data; the values are given in the following table, and those of isopentane are added for the sake of comparison:—

	Ether.	Isopentane.
R	840·34	863·56
e	7·485	7·473
k	3·188	3·636
g	4·4539	6·2318
l	5,095,070	5,420,800

The formula was tested by drawing a system of continuous isothermals giving pv plotted against $v^{-\frac{1}{3}}$, and then the experimental values were put in as dots; there is on the whole a fair agreement between calculation and experiment, as may be seen on inspection of the diagram. It is generally difficult in these investigations to know how much may be reasonably allowed for experimental errors. Fortunately in

* This formula was incorrectly stated in my former paper "On the Isothermals of Isopentane;" the correct form is that given above.

this instance we have a clue to guide us, as Messrs. Ramsay and Young in testing their linear law published tables comparing pressures found with pressures calculated (*loc. cit.* pp. 438–440 and pp. 442–445), and from these it is seen that they were willing to allow over 1 per cent. as a possible experimental error. In this connexion they remark: "It is to be noticed that the greatest divergence is at the temperatures 250° and 280°, but the deviations are in opposite directions and must therefore be ascribed to experimental error" (*loc. cit.* p. 444).

I likewise found in testing my formula that the greatest divergence is at temperatures 280°·35 C. and 250° C., and that the deviations are in opposite directions, and therefore consider it justifiable to attribute them mainly to the same cause. For the remaining temperatures discrepancies occur fairly often of over 1 per cent., but none so great as 2 per cent., so that they still seem to lie within the limits of experimental error.

Finally we may infer that both the general conclusions obtained in the former paper with regard to isopentane hold good also in the case of ether.

Discussion.

Prof. Ramsay said that experimental errors might account for some of the lack of agreement between proposed formulæ and direct observation of the behaviour of gases. Isopentane was probably a better investigated body than ether, for it was simpler. Ether tended to form complex molecular groupings, but isopentane was probably a mono-molecular liquid.

Prof. Perry did not quite agree with the author's conclusions. It was necessary to distinguish between a formula founded on a physical hypothesis and a mere empirical formula. The author had assumed that the Ramsay and Young formula was very exact; its originators did not put it forward as being infinitely exact. Probably the best test for such a formula as that under discussion would be derived from some thermo-dynamical conclusion deduced

from it. The Rose-Innes formula, with five constants and inplying discontinuity, was to be distrusted, for there was no such thing as discontinuity in the problem. In any case, an empirical formula should have a very simple form.

Mr. Rose-Innes admitted that a formula founded on sound hypothesis was to be preferred to empirical expressions. But mathematicians had not yet provided a hypothesis applicable to a substance whose molecular arrangement was so complicated as that of ether. Mathematicians must therefore improve their methods before working formulæ could be deduced from their hypotheses. The use of an empirical formula with five constants was justified by Kepler for the planetary orbits. Kepler used that formula with no other justification than his experience that an ellipse fitted his observations better than a circle. Similar instances might be cited from recent work on the theory of solution, and osmotics.

Mr. Johnstone Stoney was disposed to look for a mathematical cause for the cusp; it was improbable that the physical change was so abrupt as that represented graphically by the author. The question might be tested by plotting the two curves $y = V^{-\frac{1}{2}}$ and $y = aV^2$, and by observing whether these also suggested discontinuity.

V. *The Failure of German Silver and Platinoid Wires.*

By ROLLO APPLEYARD *.

THE object of the present paper is to direct attention to the serious mechanical defectiveness of certain alloys used for electrical wires. The question is the more important because mechanical weakness implies electrical instability. In what follows I endeavour to bring together facts enough to indicate the general behaviour of german-silver and platinoid, and especially to settle the case as regards the conditions external to the wires.

It is necessary to remember that great differences exist between different samples of the same nominal quality of alloy; so that, of a hundred pounds' weight of wire, ninety-nine may be proof against all ordinary climates and conditions to all time, while the remaining pound may become fragile under the same treatment in a few weeks.

Many suggestions have been made to me, and are yet current, as to the cause of the failure of these wires. A well-known expert in electrical matters attributes it to sulphur in the ebonite of the electrical apparatus. It will, however, be shown that failure occurs apart from all ebonite and sulphur. Paraffin-wax has been mentioned by another authority as the probable cause of these defects. Instances will be given to prove that paraffin-wax is an absorbent of moisture, and consequently useless for purposes of protection against climate; but I have no evidence that it is aggressively harmful to the wires. Chemists have variously attributed the failure to the presence of zinc, copper, antimony, arsenic, sulphur, phosphorus, and tungsten in the wires: the chemistry of the question must therefore be regarded as *sub judice*.

Tropical heat and moisture accelerate failure, but not in all cases. Moisture has been proved by Brereton Baker to be essential to certain chemical combinations; and there is evidence enough in what follows that the same agency is effective in bringing about changes in the constitution of

* Read November 26, 1897.

alloys. The deteriorating effect of heat and moisture is not limited to mixed metals; for I have recently examined a large tube made of electrolytic copper which, under the influence of steam, quickly became very seriously "pitted." Another tube, of the same electrolytic copper, became similarly "pitted" when used for conveying sea-water. When these were replaced by tubes of ordinary copper, there was no further trouble.

During the past six years some very remarkable instances of the failure of alloys have come to my notice, particularly with regard to wires of german-silver and platinoid used in the construction of resistance-coils. Specimens of these wires, insulated with white silk, were submitted to various conditions of climate; they were sent respectively to India, Brazil, Chile, Peru, Ecuador, Nicaragua, Mexico, and Texas. For comparison and reference, similar wires were in some instances kept in England. Several thousands of bobbins were thus distributed in widely different latitudes, *i. e.*, to Valparaiso, Iquique, Chorillos, Santa Elena, Panama, San Juan del Sur, Salina Cruz, Coatzacoalcos, Vera Cruz, Galveston, Pernambuco, Bahia, Rio Janeiro, Monte Video, and Calcutta.

A few years after this distribution faulty bobbins were reported from San Juan del Sur, Santa Elena, Panama, Vera Cruz, Bahia, and Calcutta. These six towns all lie on or near sea-coasts, and they are nearly on the same terrestrial isotherm, *i. e.* the isotherm including the area of high terrestrial mean temperature, 25° C.

In all cases of failure the alloy had become brittle and the wires had broken, not only at the outer layers, but also within the coils. The following is a short history of the wires that failed:—

(1) In April 1891 some german-silver wire, 16 mils diameter, doubly covered with silk, was wound on small boxwood bobbins and then treated with paraffin-wax. Each bobbin was afterwards lapped with a strip of leather. They were all packed in tin-lined soldered cases containing straw, and were shipped to Vera Cruz, on the Mexican Coast—a voyage of twenty-six days. A month after leaving England the cases were unpacked. It was then noticed that the leather lappings of the bobbins had deteriorated, the silk coverings were discoloured, the german-silver had become brittle, and breaks

had occurred in the wire. The packing-straw showed signs of dampness. The case had been stowed in the hold with other cargo in rather a hot part of the ship. No current was ever sent through this wire except the small fraction of a milliampere used momentarily while testing the resistance before shipment.

(2) In February 1891 similar bobbins, of nominally the same german-silver wire, were shipped to Valparaiso and Iquique. These were not treated with paraffin-wax, they were merely lapped with leather. During the Chilian war the apparatus was dismantled. I have had no information as to the state of these bobbins since January 1893, when the wire was reported to be in good condition. The mean temperature of Valparaiso is lower by about 10° C. than the mean temperature of Vera Cruz.

(3) In June 1895 bobbins of platinoid were shipped to the town of Bahia, on the Brazilian coast. The wire was silk-covered and treated with paraffin-wax. No leather or ebonite was used in connexion with this apparatus. An outer box of mahogany protected the bobbins from insects. At the end of a very wet and stormy season the wire failed. It was reported "faulty" in January 1896, about six months after its arrival in Brazil. When unwound, the wire showed several fractures; it had become "short" locally. Bahia lies almost on the same isotherm as Vera Cruz, 25° C.

(4) Between the months of June and August 1893 bobbins of platinoid were shipped to Valparaiso, Iquique, and Chorillos. The wire was silk-covered and treated with paraffin-wax. Each bobbin had an outer tube of ebonite. There have been no reports of failure from any of these towns. This goes to prove that the presence of ebonite is not itself sufficient to account for the fracture of platinoid wires.

(5) In November 1893 similar bobbins were shipped to Galveston and Coatzacoalcos: no faults have, so far, developed in them. Here, again, ebonite has done no evident harm. Coatzacoalcos is nearer to the equator than is Vera Cruz.

(6) In December 1894 similar bobbins were shipped to Santa Elena, on the coast of Ecuador, and to San Juan del Sur, in Nicaragua. Two years later two of the Santa Elena bobbins were reported "faulty," and the same number failed at

San Juan del Sur during the same time. Both towns are within about 10 degrees of latitude of the equator.

(7) In September 1895 similar bobbins were shipped to Panama. The wire of one was reported "broken" in November 1896.

(8) Between October 1894 and September 1895 about a thousand bobbins of platinoid wire, made and protected as above described, treated with paraffin-wax and sheathed with ebonite, were shipped to Pernambuco, Rio Janeiro, and Salina Cruz. No reports of failure have, so far, been received from these towns. They are all within the tropics.

(9) In March 1895 a voltmeter, wound with silk-covered german-silver wire, was shipped to Calcutta. The wire was not treated with paraffin-wax, and no ebonite was used in connexion with it. In February 1897 every inch of the wire was rotten. As the coil was unwound, the wire fell to pieces. Slight electrical heating had possibly accelerated the structural change of the alloy.

(10) The specimens of various german-silver and platinoid wires on bobbins kept in England are still quite good. This applies to the wire such as was used for the voltmeter, as well as to the platinoid such as was used for the resistance-coils. This autumn I selected some of these home-specimens and formed them into small coils. They were then exposed just above the surface of a tank of water that was boiled all day and allowed to cool all night. This process was continued for six weeks with no apparent deteriorating effect upon any of the coils.

(11) A piece of bare platinoid wire kept for some years in the laboratory has become discoloured, and there are several black spots in it; but I can find no "short" places and no mechanical weakness anywhere.

(12) Some suspended helices of bare german-silver wire have broken in several places; these have had current through them from time to time. One of the fractures occurred at or very near a brazed joint. The helices were under slight torsional stress. There were no fractures in the short horizontal german-silver wires connecting these helices.

The constitution of german-silver is stated to vary as follows:—

Copper	50–66 parts.
Zinc	19–31 „
Nickel	13–18 „

Platinoid is generally described as german-silver with two per cent. of tungsten. According to Brannt, the tungsten, in the form of phosphor-tungsten, is first melted with a certain quantity of copper. The nickel is next added, then the zinc, and finally the remainder of the copper. In order to remove phosphorus and a portion of the tungsten, both of which separate as dross, the resulting compound is several times remelted. It is probable that traces of arsenic and phosphorus are present in the alloy.

If nickel is in too great proportion in nickel-copper alloys, oxygen is absorbed during fusion, and liberated on cooling. The result is a porous metal. Again, if the temperature of casting is too high, or if the cooling is irregular, cavities may be expected in the final alloy.

The surprising diminution in tensile strength produced by traces of impurities, for some alloys, has been studied by Prof. Roberts-Austen and other metallurgists. A very small percentage of arsenic in nickel-copper-zinc compounds causes extreme brittleness. So also does a small addition of lead or iron. And as arsenical nickel ore is the source of much of the nickel of commerce, the failure of german-silver and platinoid may possibly be due to traces of arsenic.

At this point, however, I propose to discriminate between two kinds of brittleness, my object being to simplify the discussion of the problem. That such a distinction is necessary will perhaps be best illustrated by an example. Prof. Roberts-Austen demonstrated that the addition of 1 per cent. of lead reduces the tensile strength of gold by more than two-thirds. Similarly, he observed that arsenic renders gold very fragile, and that 0·2 per cent. of bismuth *instantly* converts gold into an alloy that crumbles under the die. In all these cases, brittleness is characteristic of the alloy from the moment of solidification; it may, in fact, be regarded as a definite function of the atomic volumes of the constituent elements. I propose to call this "primary" brittleness.

But the brittleness of german-silver and platinoid is of a

different order. It is a *subsequent* phenomenon. For example: the alloys from which electrical wires are made are necessarily strong in the first instance; if they were mechanically weak, *i. e.*, if they possessed "primary" brittleness, they would fail in the process of "drawing" through the die. Hence, the distinction between "primary" and "secondary" brittleness. The one is an accident of birth, the other is a disease that develops with age and circumstance.

The fracture of platinoid, as seen under the microscope, is granular or crystalline. In new and good specimens the colour is silver-grey, and uniform except for a few well-defined patches of brown. Occasional specimens of brittle wire retain, in parts, the silver-grey fracture. As a rule, however, bad specimens have, encroaching on the silver-grey area, patches of purple, yellow, copper-colour, and brown, with frequent cracks and fissures that communicate more or less with the surface of the wire. Very bad specimens have, at times, scarcely any of the silver-grey colour at the fracture. The patches of dark-purple, yellow, and copper-colour are clearly defined, as though each of them represented a distinct alloy, or a separate mass of "liquated" metal. On a particular bobbin, brittleness is often restricted to a foot or two of the wire. Similar remarks apply to the appearance of the fractures of german-silver.

The crevices and fissures in bad specimens are easy to observe with a microscope. They are probably developed during wire-drawing, as the result of pores in the cast metal. Another explanation of them may be looked for in the "liquation" of some of the metals constituting the alloy. By this action, the portion of the alloy that first solidifies rejects the yet molten portions, as ice rejects foreign matter. As cooling continues, the various constituents become isolated, homogeneity is lost, and I think we may fairly assume that, in consequence, the strength of the material varies from point to point of its mass, so that in passing afterwards through the die, the weaker constituents give way, and the general structure is loosened. Crystallization and internal electrical actions may also result in local weakness, crevices, and fissures.

Keeping in mind the existence of these fissures it is a

simple matter to account for the fact that wires deteriorate more quickly under a small stress than when they are submitted to no stress. Apart from the mechanical weakening, it is evident that any extending force opens the crevices and makes way for air and moisture—it may also facilitate crystallization. And clearly, if the wires are being used for electric currents, as is the case with resistance-coils on arc-light circuits, great local heating occurs at these weakened sections, and fracture is inevitable. This was probably the fate of the voltmeter-wire mentioned in example (9) above. It was almost certainly the fate of the suspended helices (12).

From what precedes, there is plenty of evidence that secondary brittleness is fostered by tropical heat and moisture. A good wire, free from incipient cracks, may last for years, or may be proof against heat and moisture, as were the wires in example (10). But if there are any fissures, there is no doubt that moisture intrudes upon the alloy through these capillary channels, with destructive effect.

There is a kind of tradition that paraffin-wax is a perfect safeguard against moisture; as a matter of fact it is highly absorbent. The wax on the bobbins returned from the tropics is completely choked with moisture. Shellac, or the old-fashioned sealing-wax varnish, resists much better. There is no evidence to show that ebonite is harmful to german-silver or platinoid, but it may be well to keep the metal out of actual contact with ebonite.

Can metallurgists tell us the difference, in constitution and structure, between a german-silver wire that decays in four weeks, and another that under similar conditions never fails? Or, what is even more important, can they make us platinoid that shall never fail? If they cannot, it becomes necessary to surrender those cheaper and better electrical materials, and fall back upon the more expensive alloys, beginning at platinum-silver. If sufficient time and means were at the disposal of metallurgists they might discover the secret of permanence in alloys. Germany, with the advantages of a National Laboratory, has already attacked the question, and "manganin" is the result. Its adoption there as a satisfactory alloy is directly due to work done upon it by the Reichsanstalt. But it has yet to be proved that manganin

will endure the conditions imposed by the tropics. Moreover, the doubt arises as to whether it is desirable to obtain from abroad material that ought to be produced by our own country. British cable-manufacturers are already importing thousands of tons annually of sheathing-wire from Germany; and it seems probable that, for the want of a National Laboratory, instrument-makers will now get their resistance-wire from that same adventurous foreign source.

Discussion.

Prof. Ayrton said the paper had raised the extremely interesting question of the permanence of metals used for resistance-coils. Some time ago he had immersed bare platinoid wires in running water in metal tanks, and the wires all broke in short pieces. He thought, at the time, this might be due to electrolysis. On another occasion he had found that, by raising the temperature of platinoid to a dull-red heat in the air, by an electric current, any acquired faults in the wire were corrected, and the original resistance and flexibility were restored. Even when such metals are in good condition, the resistance-temperature curve does not return upon itself; it encloses a loop, indicating two distinct values for resistance at each temperature. He had been told by Dr. Muirhead that coils intended for hot climates should be enclosed in air-tight metal cases. English manufacturers were still dubious in regard to manganin. In 1892 he had twenty coils made of this material, each of 1000 ohms; the wire was silk-covered. There were 2000 volts between the terminals. Their resistance had certainly not changed by 1 in 1000, although there was some amount of vagueness regarding the fifth figure, which might be due to molecular alteration, for they were heated more than was good for resistance-coils. He confessed that this manganin had come from Germany.

Dr. S. P. Thompson mentioned an alloy that was proposed in Germany under the name of "Constantine." He would like to know whether any information could be obtained as to the employment of cast-iron wire. It was a metal that in some respects commended itself. He had observed the

failure of some german-silver coils, but he had generally attributed it to rough handling.

Mr. W. Watson referred to the recent work done at the Reichsanstalt with regard to german-silver and platinoid. It was there found that all alloys containing zinc were liable to erratic changes of resistance, and were unsuitable for standard coils. Moreover, even the slight amount of zinc introduced into manganin during soldering with soft solder robbed that alloy of its constancy. Silver solder, containing 75 per cent. of silver, should be employed for manganin. If Prof. Ayrton's coils were soldered with soft solder, that was sufficient to account for the change in the fifth figure. Shellac varnish was undoubtedly the best protection for coils. Absolute alcohol should be used as the solvent, and the coils should afterwards be heated for some hours at 140° C. If a heating-current was passed through german-silver or platinoid coils immersed in water, the general result was to produce brittleness. Mr. Watson then described a thermostat which he had contrived for "ageing" the coils after applying the shellac varnish. A hot-air oven contains a thermometer with a platinum contact at the 140° mark and an 8 c.p. lamp. The thermometer is in circuit with a relay actuating a mercury-key for the 8 c.p. lamp. The key consists of two mercury-cups, and a corresponding U-piece of copper, inverted, one limb to each cup. It is important to keep the heating-circuit always made; for this purpose a 32 c.p. lamp is permanently connected between the two cups.

VI. *On Lord Kelvin's Absolute Method of Graduating a Thermometer.* *By* J. ROSE-INNES, *B.Sc.**

IN a paper "On the Thermal Effects of Fluids in Motion" Lord Kelvin has given the cooling effects exhibited by various gases in passing through a porous plug; and he found that the effects for any one gas kept at the same initial temperature were proportional to the difference of pressure on the two sides of the plug (Reprinted Papers, vol. i. pp. 333–455). He also found that the cooling effect for any one gas per unit difference of pressure varies as the inverse square of the absolute temperature; and this rule succeeds very well in the case of air. For carbonic acid, however, the results furnished by this rule are not so satisfactory, as may be seen by inspecting the table given by him comparing the actual with the theoretical cooling effect (*loc. cit.* p. 429). Moreover in the case of hydrogen it is found that there is a *heating* effect, which increases, if anything, when the temperature rises, so that here the law of the inverse square of the temperature is wholly inapplicable. It seemed to me that it might be possible to hit upon some simple algebraic expression which should reproduce the experimental results rather better than Lord Kelvin's rule does; and in fact it was found that a satisfactory agreement between observation and calculation might be obtained by putting

$$\text{cooling effect} = \frac{\alpha}{T} - \beta,$$

where α and β are constants characteristic of the gas, and T is the absolute temperature.

The following values of α and β were found from the experimental data:—

	α.	β.
Air	441·5	·697
Carbonic acid . .	2615	4·98
Hydrogen . . .	64·1	·331

A comparison of the actual results with those calculated by the new formula is given in the following table:—

* Read December 10, 1897.

Name of Gas.	Temp.	Actual cooling effect.	Calculated cooling effect (Kelvin's formula).	Calculated cooling effect (New formula).
Air	0°	0°92	°92	·920
	7·1	·88	·87	·879
	39·5	·75	·70	·716
	92·8	·51	·51	·510
Carbonic Acid.....	0	4·64	4·64	4·60
	7·4	4·37	4·40	4·35
	35·6	3·41	3·63	3·49
	54·0	2·95	3·23	3·02
	93·5	2·16	2·57	2·16
	97·5	2·14	2·52	2·08
Hydrogen .	4·5	−0·100	.....	−·100
	91·0	− ·155	.....	−·155

It will be seen that the formula proposed in this paper, regarded simply as an empirical formula, is more efficient than Lord Kelvin's; there is nothing astonishing in this as it contains two disposable constants instead of only one. But it has the following further advantages:—

(i.) It includes the three cases of air, hydrogen, and carbonic acid under one form, and therefore enables us to treat them all in one common investigation.

(ii.) It renders more manageable the differential equation concerned in the thermodynamic scale of temperature, and leads to simpler algebraic results after integration.

This last proposition we must now proceed to prove. It is shown by Lord Kelvin that when a gas passes through a porous plug we must have

$$t\frac{dv}{dt} - v = \frac{\mathrm{JK}}{\Pi}\theta,$$

where t and v denote the temperature and volume of the gas respectively, K its specific heat, θ the cooling effect per atmo of differential pressure, Π the value of one atmo, and J the value of Joule's mechanical equivalent (Reprinted Papers, vol. iii. p. 179). Hence

$$\frac{1}{t}\frac{dv}{dt} - \frac{v}{t^2} = \frac{\mathrm{JK}}{\Pi}\frac{\theta}{t^2}$$

$$\frac{d}{dt}\left(\frac{v}{t}\right) = \frac{\mathrm{JK}}{\Pi}\left\{\frac{\alpha}{t^3} - \frac{\beta}{t^2}\right\}.$$

Estimate of the Absolute Value of the Freezing-point of Water.

Integrate the last equation between the limits t_0 and t_1, and we obtain

$$\frac{v_1}{t_1}-\frac{v_0}{t_0}=\frac{JK}{\Pi}\left\{\frac{\alpha}{2t_0^2}-\frac{\alpha}{2t_1^2}-\left(\frac{\beta}{t_0}-\frac{\beta}{t_1}\right)\right\}$$

$$=\frac{JK}{\Pi}\left\{\frac{1}{t_0}-\frac{1}{t_1}\right\}\left\{\frac{\alpha}{2}\left(\frac{1}{t_0}+\frac{1}{t_1}\right)-\beta\right\}$$

$$=\frac{JK}{\Pi}\left\{\frac{1}{t_0}-\frac{1}{t_1}\right\}\frac{\theta_1+\theta_0}{2},$$

where θ_1 and θ_0 are the values of θ at the temperatures t_1 and t_0 respectively.

Multiply the equation by $t_0\,t_1$ and we get

$$v_1\,t_0-v_0\,t_1=\frac{JK}{\Pi}(t_1-t_0)\frac{\theta_1+\theta_0}{2}$$

$$(v_1-v_0)t_0-v_0(t_1-t_0)=\frac{JK}{\Pi}(t_1-t_0)\frac{\theta_1+\theta_0}{2}.$$

Hence

$$t_0=\frac{t_1-t_0}{v_1-v_0}\left(v_0+\frac{JK}{\Pi}\frac{\theta_1+\theta_0}{2}\right)$$

$$=(t_1-t_0)\frac{v_0}{v_1-v_0}\left(1+\frac{JK}{\Pi v_0}\frac{\theta_1+\theta_0}{2}\right).$$

If t_1 and t_0 are taken as the boiling-point and freezing-point of water respectively, then this equation gives us the value of the freezing-point t_0 in terms of the interval t_1-t_0; it is usual, as pointed out by Lord Kelvin, to take the interval t_1-t_0 as containing 100 degrees (*loc. cit.* p. 175). It is evident that we should have obtained the same value for t_0 if θ had been constant throughout the range of temperature t_0 to t_1, and equal to $\frac{\theta_1+\theta_0}{2}$. This shows that the proper mean cooling-effect is simply the arithmetic mean of th cooling effects at the boiling-point and freezing-point. The following table gives us the value of the freezing-point derived from experiments on the three gases.

	Uncorrected estimate of temperature of freezing-point.	Correction.	Corrected estimate.
Hydrogen . . .	273·13	−·13	273·00
Air	272·44	·72	273·16
Carbonic acid . .	269·5	4·35	273·85

The first column of figures is taken from Lord Kelvin's paper (*loc. cit.* p. 177).

Thermodynamic Correction for a Constant-pressure Gas Thermometer.

Suppose now we have a temperature t lying above both t_1 and t_0, and fixed by some definite physical phenomenon. We require to know exactly how it lies with respect to t_0 and t_1.

We start as before with the differential equation

$$\frac{d}{dt}\left(\frac{v}{t}\right) = \frac{\mathrm{JK}}{\Pi}\left\{\frac{\alpha}{t^3} - \frac{\beta}{t^2}\right\}.$$

We may put this

$$\frac{d}{dt}\left(\frac{v}{t}\right) = \frac{\mathrm{A}}{t^3} - \frac{\mathrm{B}}{t^2},$$

if $\mathrm{A} = \dfrac{\mathrm{JK}\alpha}{\Pi}$ and $\mathrm{B} = \dfrac{\mathrm{JK}\beta}{\Pi}$

Integrate between the limits t_0 and t,

$$\frac{v}{t} - \frac{v_0}{t_0} = \frac{1}{2}\left(\frac{\mathrm{A}}{t_0^2} - \frac{\mathrm{A}}{t^2}\right) - \mathrm{B}\left(\frac{1}{t_0} - \frac{1}{t}\right)$$

$$= \left(\frac{1}{t_0} - \frac{1}{t}\right)\left\{\frac{\mathrm{A}}{2}\left(\frac{1}{t_0} + \frac{1}{t}\right) - \mathrm{B}\right\}.$$

Hence

$$\frac{v}{t} - \frac{v_0}{t_0} = \frac{-t_0}{tt_0}\left\{\frac{\mathrm{A}}{2}\left(\frac{1}{t_0} + \frac{1}{t}\right) - \mathrm{B}\right\}.$$

Multiply by $t_0(t-t_1)$ and we shall have

$$v\left(t_0 - \frac{t_0 t_1}{t}\right) - v_0(t-t_1) = \frac{(t-t_1)(t-t_0)}{t}\left\{\frac{\mathrm{A}}{2}\left(\frac{1}{t_0} + \frac{1}{t}\right) - \mathrm{B}\right\}.$$

Similarly by interchanging t_0 and v_0 with t_1 and v_1, we might obtain

$$v\left(t_1 - \frac{t_0 t_1}{t}\right) - v_1(t-t_0) = \frac{(t-t_1)(t-t_0)}{t}\left\{\frac{A}{2}\left(\frac{1}{t_1}+\frac{1}{t}\right) - B\right\}.$$

Subtract the equation last but one from the last,

$$v(t_1-t_0) - (v_1-v_0)t + v_1 t_0 - v_0 t_1 = \frac{(t-t_1)(t-t_0)}{t}\frac{A}{2}\left(\frac{1}{t_1}-\frac{1}{t_0}\right).$$

We may write this

$$(v-v_0)(t_1-t_0) - (v_1-v_0)(t-t_0) = \frac{(t-t_1)(t-t_0)}{t}\frac{A}{2}\left(\frac{1}{t_1}-\frac{1}{t_0}\right).$$

Divide by v_1-v_0 and transpose,

$$t-t_0 = \frac{v-v_0}{v_1-v_0}(t_1-t_0) + \frac{(t-t_1)(t-t_0)}{t}\frac{A}{2(v_1-v_0)}\left\{\frac{1}{t_0}-\frac{1}{t_1}\right\}.$$

Finally,

$$t = t_0 + \frac{v-v_0}{v_1-v_0}(t_1-t_0) + \frac{JK(\theta_0-\theta_1)}{2\Pi(v_1-v_0)}\frac{(t-t_1)(t-t_0)}{t}.$$

This last expression for t may be said to consist of three parts :—

(i.) There is the quantity t_0, whose absolute value may be considered to have been determined once for all by means of its own equation. We may regard it as known numerically with sufficient accuracy.

(ii.) There is the term $\frac{v-v_0}{v_1-v_0}(t_1-t_0)$. This term gives us the degrees above freezing-point on the equi-expansion method of graduation. It is what is usually called the "temperature."

(iii.) The quantity $\frac{JK(\theta_0-\theta_1)}{2\Pi(v_1-v_0)}\frac{(t-t_1)(t-t_0)}{t}$ is due entirely to the Joule-Thomson effect. It may be regarded as a correction necessary owing to the deviation of the substance from a perfect gas. It is this quantity which is calculated and tabulated so as to give us the means of arriving at the absolute scale. We notice that in order to calculate this term, we require to know the value of t, the very quantity we are seeking to find. Sufficiently accurate, however, for the purpose of calculating this small term, will be the value of t found by means of a first approximation.

Thermodynamic Correction for Constant-volume Gas-Thermometer.

We will now compare the indications of a gas-thermometer kept at constant volume with the thermodynamic scale of temperature.

Let us start as before with the differential equation

$$\frac{d}{dt}\left(\frac{v}{t}\right) = \frac{A}{t^3} - \frac{B}{t^2}.$$

Integrate this between the limits t and ∞ and we have

$$P - \frac{v}{t} = \frac{1}{2}\frac{A}{t^2} - \frac{B}{t},$$

where P is the value of $\frac{v}{t}$, as v and t are made to grow indefinitely large with p constant. To determine the form of P we must appeal to experiment. We know that when a gas is made to expand to larger and larger volumes it obeys Boyle's law more and more closely; hence we infer that when v and t are made indefinitely large, the value of $\frac{v}{t}$ must vary inversely as the pressure. We may therefore write

$$P = \frac{C}{p}$$

where C is a constant characteristic of the gas.

Writing in this value we have as the complete solution

$$\frac{C}{p} - \frac{v}{t} = \frac{1}{2}\frac{A}{t^2} - \frac{B}{t}.$$

Multiply this by pt, and transpose,

$$pv = Ct - p\left(\frac{1}{2}\frac{A}{t} - B\right).$$

If we neglect the Joule-Thomson effect we have as a first approximation $pv = Ct$, or $t = \frac{pv}{C}$, which is the value for t in the case of a perfect gas; and this approximate value for t may be used in the term involving the Joule-Thomson effect on the right-hand side of the equation. We then get

$$pv = Ct - \frac{CA}{2v} + pB.$$

This equation represents our *second* approximation to the correct formula. We may also write it

$$p(v-\mathrm{B}) = \mathrm{C}t - \frac{\mathrm{CA}}{2v}.$$

Imagine now that we keep v constant, and use the equation to determine t when p is measured. Let the suffix 0 refer to the freezing-point, and the suffix 1 to the boiling-point as before. We then have

$$p_0(v-\mathrm{B}) = \mathrm{C}t_0 - \frac{\mathrm{CA}}{2v},$$

$$p_1(v-\mathrm{B}) = \mathrm{C}t_1 - \frac{\mathrm{CA}}{2v}.$$

By subtraction

$$(p_1-p_0)(v-\mathrm{B}) = \mathrm{C}(t_1-t_0),$$

$$(p-p_0)(v-\mathrm{B}) = \mathrm{C}(t-t_0),$$

$$\therefore \quad \frac{p-p_0}{p_1-p_0} = \frac{t-t_0}{t_1-t_0};$$

or

$$t = t_0 + \frac{p-p_0}{p_1-p_0}(t_1-t_0).$$

To the degree of approximation to which we are working, therefore, there is no thermodynamic correction needed for a constant-volume gas thermometer. There may be a correction involving *squares* of small quantities, which would appear on a nearer approximation. Such a correction, however, would not be worth taking into account in the case of a thermometer constructed with air or hydrogen, as the unavoidable errors of experiment would certainly be much larger than the correction. It is satisfactory to know that for all practical purposes absolute temperature is to be obtained with very great accuracy from Regnault's thermometric system by simply adding the value of t_0 to his numbers for temperature on the centigrade scale.

This result differs from that obtained by Rowland *, who employed Lord Kelvin's law of the inverse square of the temperature, and inferred that there was a correction needed

* Proceedings Amer. Acad. Arts & Sciences. xv. (n. s. vii.) p. 114.

involving the first power of the cooling-effect. Both his investigation and my own involve the assumption that an empirical formula found to hold through a short range of temperature can be used for any temperature however high, and hence neither his result nor mine is conclusively established ; but it seems interesting to show that the employment of a new expression, at least as good as Lord Kelvin's, for the cooling-effect, leads to a smaller value for the correction.

Discussion.

Dr. S. P. Thompson said the empirical expression,

$$\left(\frac{\alpha}{T}-\beta\right),$$

indicated that at some particular temperature the cooling effect vanished ; that was a point suggestive of useful results if investigated by experiment.

Mr. J. Walker read a communication from Mr. Baynes on the paper, and remarked on the desirability of adopting two constants. He thought that further experiments should be made to discover how specific heat at a constant temperature depends on temperature. The calculated values for hydrogen were too few to be taken as evidence of the validity of the rule.

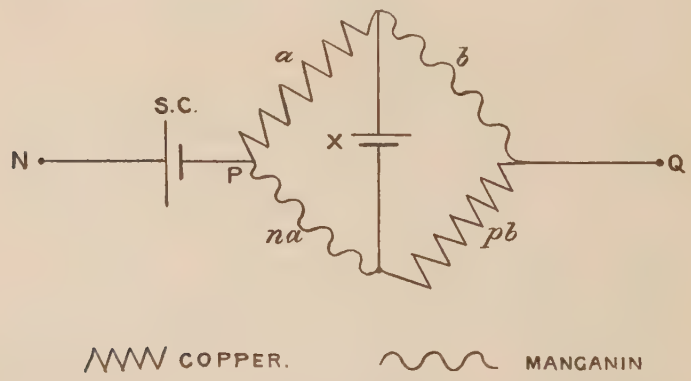
Mr. Rose-Innes, in reply, said that from what was known of hydrogen, it might be expected to behave at ordinary temperatures as air behaves at higher temperatures. His object was, if possible, to include in one formula the case of the three investigated gases. This was much better than having a separate formula for each gas. Whether or not hydrogen was confirmatory with air and carbonic acid might be considered as *sub judice* ; it required further experimental data to test the formula in that case.

VII. *On Apparatus for Self-acting Temperature Compensation of Standard Cells.* *By* Albert Campbell, *B.A.**

In ordinary testing work when using a Clark cell as the working standard of difference of potential, it is desirable to get rid of the trouble of continually having to make the temperature correction. Some time ago ('The Electrician,' pp. 601–603, September 6, 1895) I proposed and investigated three methods by which a self-acting correction could be attained. Quite recently I have constructed the apparatus for two of the methods, and I have found it an easy matter to reach the accuracy aimed at (viz. maximum error 1 in 2000).

The arrangement of the first piece of apparatus is shown in fig. 1.

Fig. 1.



S.C. is the standard cell. Four resistances of values a, b, na, and pb are connected as in a Wheatstone's bridge with an auxiliary cell X, whose voltage must be known within 5 or 6 per cent. A Leclanché cell is found suitable for this. The resistances a and pb are of a metal such as copper or iron with a large temperature-coefficient, while b and na are of manganin or a similar alloy. As shown in the paper cited above, we can choose n and p so as to have a constant voltage of, say, 1·400 volts between N and Q at all temperatures from 0° to 20° C. when S. C. is a Clark cell. The arrangement thus not only corrects for temperature but can be made to

* Read December 10, 1897.

give a more even figure by cutting off the 0·034 volts at the same time. In one of the coils exhibited the resistances have the following values at 15° C :—

$a = 48{\cdot}46$ ohms.
$na = 482{\cdot}7$ „
$b = 63{\cdot}63$ „
$pb = 495{\cdot}0$ „

As the absolute values of a and b are of no importance so long as they are high enough to keep the auxiliary Leclanché cell from running down, the copper coils a and pb did not require to be adjusted but were merely measured ; their resistances were reduced to 15° C. and the manganin coils na and b were then adjusted to give $n = 9{\cdot}96$, $p = 7{\cdot}78$*. It is clearly a great saving of trouble not to have to adjust the coils which have a high temperature-coefficient. The four coils were wound on a brass cylinder 7 or 8 centim. in diameter, and then well protected with silk ribbon, four leads being brought out. To test the whole directly the cylinder well, wrapped in wadding, was placed in a tin canister, the lid of which was then *soldered on* so as to be watertight. The leads were brought out through a long tube soldered to the lid. A thermometer also passed down this tube into the canister. A Leclanché cell was connected as in fig. 1. The tin was then well immersed in water at various temperatures, and steady temperatures inside the tin were got by stirring the water until a thermometer in it showed the same temperature as the one inside the tin. The potential-difference from P to Q was measured by a potentiometer method at various temperatures with the following results :—

Temperature.	P.-D., volt.
15·5 C.	0·0338
34·2 C.	0·01472
48·5 C.	0·0000

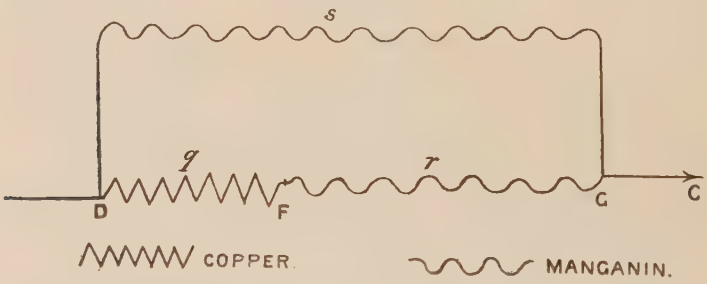
* The numerical values of p and n given in my paper mentioned above are not quite right owing to a slip in signs in the equation there given.

It will be seen that the actual observed values of the auxiliary voltage are just what would be required along with the Clark cell to give 1·400 volts at all ordinary temperatures.

The arrangement in fig. 1 was chosen so as to give the maximum possibility of correction. Mr. C. W. S. Crawley has pointed out to me that some trouble is saved in the construction of the coils by making only one of the four of copper. This also simplifies the calculation of the ratios n and p, and still gives sufficient variation to balance that of a Clark cell. It is convenient to place the cell inside the wide brass tube on which the coils are wound.

In the other arrangement exhibited three resistance-coils are connected as in fig. 2.

Fig. 2.



The resistances r and s are of manganin, and q is of copper. A current C is sent from D to G from an outside source. The values of q, r, and s are so chosen that a fixed value of C produces *at all temperatures* a potential-difference from F to G which would balance the voltage of the standard.

In the specimen shown 3·75 $q=r+s$ and $r=s$. The actual values at 15° C. are

$$r=s=3\cdot6335 \text{ international ohms.}$$
$$q=1\cdot9382 \quad \text{,,} \quad \text{,,}$$

A current of 1·000 ampere gives a balance with a Clark cell at F and G at all ordinary temperatures (to within 1 in 2000). This definite current, obtainable at all temperatures, can be passed through known resistances and so used to measure unknown voltages by potentiometer methods.

If the resistance r be subdivided other exact currents such as 2, 3, 4 ... amperes can be got directly.

It should be noted that although I have made special reference to the Clark cell, either of the two forms of compensator may be arranged for any other type of standard cell so as to wipe out the greater part of the temperature variation. For instance, if a Carhart 1 volt cell has a coefficient of 0·0097 per cent. per degree C., used with a compensator it would not vary by more than 0·001 per cent. per degree. It will thus be seen that in some cases a compensating arrangement such as I have described above would prove of value not only in ordinary testing work but also in measurements where much higher accuracy is required.

Discussion.

Mr. Swinburne said that twelve or thirteen years ago he had given a good deal of thought to compensation by wires of different temperature-coefficients. The first thing he tried was a Wheatstone's bridge. This was compensated by making the bridge-arms of wires whose temperature-coefficients differed—as, for instance, platinoid and copper. He then applied the same principle to the compensation of standard cells, using a potentiometer method that gave direct readings, and to the compensation of voltmeters and Wattmeters. These results were published between 1885 and 1890, in the electrical journals. He believed that Mr. Evershed had also developed this idea, by putting "back" turn on voltmeters, and by other differential devices. The details of Mr. Campbell's apparatus had a few points of special interest. The way in which he connected up the bridge (3) seemed particularly worthy of notice.

Prof. Ayrton asked whether thermo-electric effects produced difficulty in the compounded arrangement.

Mr. Campbell said the system was symmetrical, and the thermal currents were consequently neutralized.

Mr. Campbell, in reply, said that he was surprised to hear Mr. Swinburne justify the retention of the uncompensated Clark cell because it is always used with a potentiometer.

He ought to have said "with a potentiometer *and a thermometer.*" This means that every time the potentiometer current is re-set a reading of the temperature has to be taken and the slider adjusted to correspond to this. This double operation, which must necessarily increase the probable error, is entirely done away with if the cell is compensated. If any part of a measurement can be rendered automatic, surely we may expect to gain both in accuracy and in quickness of work. With reference to the second apparatus, it is *not*, in itself, a compensated potentiometer, and so does not seem to be the same as that Mr. Swinburne invented some years ago. It is simply an arrangement by which a current can be set to an exact value (say 0·1 ampere) with the help of a standard cell. This current can of course be sent through resistances of invariable material, and so potentiometer measurements can be made.

VIII. *Variations in the Electromotive Force of the* H-*form of Clark Cells with Temperature.* *By* F. S. Spiers, *B.Sc.*, F. Twyman, *and* W. L. Waters.*

The work described in this paper was intended as a completion of the investigation carried out some years ago by Prof. Ayrton and Mr. W. R. Cooper † on the effect of variations of temperature on the E.M.F. of the Board of Trade form of Clark cell. Their results showed that it was not possible, by applying the ordinary temperature-correction, to obtain the true value of the E.M.F. of the cell with a greater accuracy than 0·1 per cent. It was thought that the H-form of cell would show some improvement over the ordinary type in this respect; and this was confirmed by some results published recently by Dr. Kahle ‡. The experiments to be described in this paper were undertaken with the object of seeing how far this was correct, and in general to investigate

* Read November 12, 1897.

† Proc. Roy. Soc. December 1895.

‡ *Zeitschrift für Instrumentenkunde*, August 1893.

the behaviour of this form of cell under variations of temperature. The experiments were carried out at the Central Technical College.

The tests were made on four cells set up in accordance with the specification issued by Dr. Kahle from the Reichsanstalt*. The cells were in every way alike †, and were mounted together inside a glass vessel in paraffin oil, and during the experiments they each underwent precisely the same treatment. The diameter of each of the two legs of the cell was about $\frac{3}{4}$ inch, and their length about 1 inch.

The method used for the determination of the E.M.F. was as follows. Current from a storage-cell traversed a potentiometer-wire in series with a resistance, the value of which was such that the fall of potential along the wire amounted to one ten-thousandth of a volt per centimetre-length. The cell to be tested was joined up in opposition to the standard, and the difference in their E.M.Fs. was balanced against the fall of potential along a certain length of the stretched wire.

The standard used in comparison consisted of one or other of two cells of the Muirhead type, which had been found to have an E.M.F. approximately constant during several years. The two cells were in one case, and during the experiments were placed in a thermostat in which the temperature could be kept constant to $0°{\cdot}01$ C. The E.M.F. of the standard was determined absolutely by balancing it against the P.D. between the terminals of a resistance of known value of about three ohms, due to a current of about half an ampere passing through it. The resistance had been made specially for the purpose of manganin wire, wound upon an ebonite frame and immersed in paraffin oil. It had been previously aged by heating, and had assumed a steady value, as shown by comparison with standard manganin coils. The current was measured by a Kelvin centiampere balance, the constant of which had been frequently checked by means of a silver voltameter. By this means the E.M.F. could be determined absolutely, correct to $0{\cdot}0001$ volt.

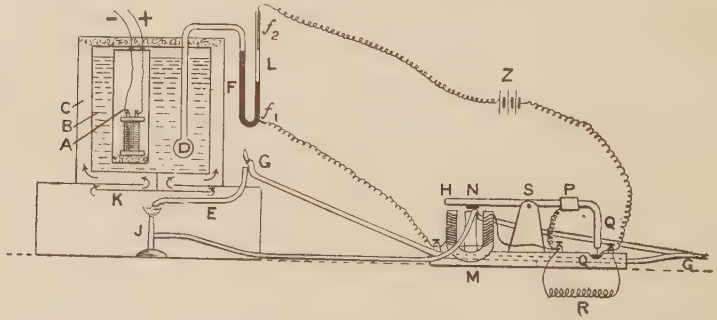
* 'Electrician,' vol. xxxi. p. 265, July 1893.

† They were set up by Mr. Cooper, Nos. 1, 2, and 4 on 19th March, 1895, and No. 3 on 12th February, 1896.

The thermostat consisted of an inner air-space, in which was placed the standard, surrounded by an outer water-bath which was heated by a gas-jet, the gas being automatically cut off when the temperature rose to a given value, and being allowed to come on again as soon as the temperature had fallen below that value.

In fig. 1, A B C are zinc vessels: A is the air-bath, containing the standard resting on cottonwool ; B the surrounding water-bath, closed at the top and containing the thermometer D ; C the enclosing jacket, between which and B the hot air from the gas-jet passes, heating the water-jacket on its way. D is an alcohol thermometer with a mercury-index F, which, on the expansion of the alcohol, is pushed up the narrow tube L, and electrically connects the two platinum wires f_1 and f_2. J is the gas-jet for heating the water-bath ; K a baffle-plate over the gas-jet, to prevent the flame heating the water-bath directly. E is an escape-tube, which serves to carry off some of the heat of the jet ; G is a little by-pass,

Fig. 1.



burning at the mouth of the tube E, and serves to relight the main jet when extinguished. M is an electromagnet ; H, its armature, is pivoted at S and weighted at P, and carries a platinum wire Q which dips into the mercury cup Q′.

When the temperature of the water-bath has reached a certain arbitrary value, the mercury-index electrically connects f_1 and f_2, completing the circuit of the electromagnet M and the battery Z. The electromagnet being thus excited, attracts its armature H, which compresses the indiarubber

tube T that feeds the main gas-jet J, between the brass piece N and the adjustable wooden block N′, and so extinguishes the light. The water-bath now cools slowly till its temperature has fallen below the fixed value, and the contact is broken at f_2 by the falling of the mercury-index. When this happens, the armature of the electromagnet is released and pulled up at once by the weight P, the gas coming on again at the jet J ; some of the gas passing up the tube E is ignited at the top by the by-pass G, and then striking back, lights the main jet J, restarting the heating, and the cycle is repeated.

As it requires a less current to hold the armature down than it takes originally to attract it, the current circuit of the magnet is so arranged that when the armature is attracted it breaks the direct-current circuit at the mercury cup Q′, and inserts into the circuit a resistance of 50 ohms, thus economizing the energy.

The temperature at which the cut-off acts can be adjusted several degrees by altering the distance which the wire f_2 projects down the tube L.

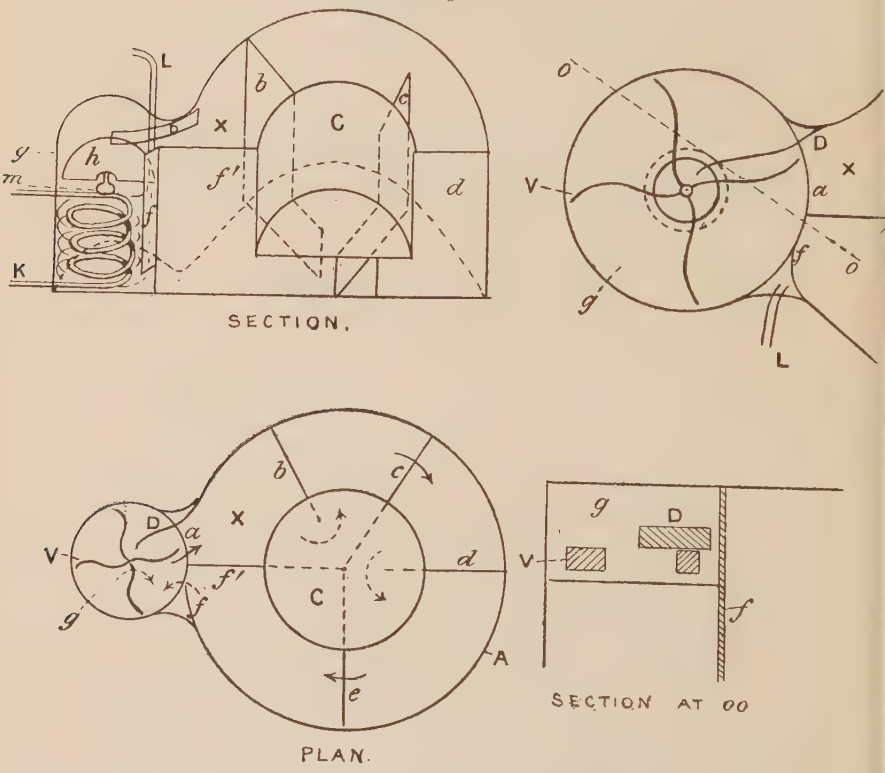
By carefully adjusting the size of the jet, the by-pass, and the height of the block N′, we could keep the temperature of the air-bath constant to 0°·01 C., the gas being cut off and coming on again about every five minutes.

To investigate the effect of temperature on the cells, it was necessary to be able to vary their temperature so as to get a uniform rise or fall. To effect this, some special form of heating-bath was needed. In its final form, this heating-bath consisted of an outer zinc bath, in which the vessel containing the Clark cells was placed, which was divided up by means of a number of partitions, reaching from the bottom of the vessel to above the surface of the oil. The oil was forced alternately under one partition and over the next, round the whole of the bath, and from the last compartment was drawn off into a separate chamber containing a heating-coil and a coil of lead tubing for an iced-water circulation, and from this chamber it was again circulates round the compartments by means of a set of centrifugal vanes.

A (fig. 2) is the outer zinc bath, containing the oil, and divide

up into compartments by means of the zinc plates, $a, b, c, \ldots$ C, the inner glass vessel, contains the cells, and rests about one and a half inches from the bottom of the outer bath on the parts of the partitions $a, b, c, \ldots$ which reach to the centre. g is the separate compartment containing the heating-coil L, the coil of lead tubing K, and the centrifugal pumping-vanes V. The vanes V are directly coupled to a motor with a

Fig 2.



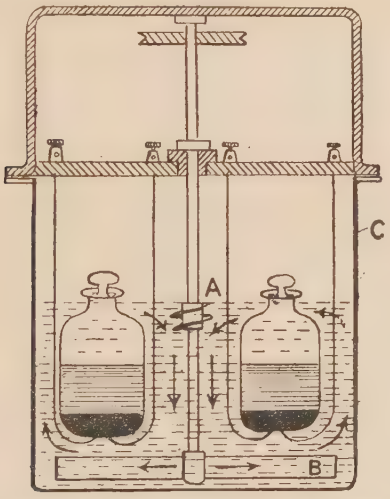
vertical shaft, this being found to be the most satisfactory way of driving them. The ring h separates the chamber g into two parts: in the upper, the vanes work and the oil is rotating; in the lower, the heating and cooling coils are placed, the oil in this part not rotating. m is a distributing plate under the hole in the centre of the ring h, which makes the circulating oil pass over the heating and cooling coils.

D is a stationary guide vane which helps to deflect the rotating oil into the next chamber X.

The oil passes under the division *b*, over *c*, under *d*, over *e*, and finally under *f* into the heating-chamber *g*, where it passes over the heating-coil, and is sucked up through the hole in the plate *h*, and, being thrown out by the vanes V, is again circulated.

The circulation obtained by this means was excellent. When the bath was tested at a rate of change of temperature of 1° in eight minutes, there was found to be a maximum difference of temperature between any two points of the bath of 0°·2 ; while at a rate of change of 1° in fifteen minutes, no difference in temperature was detected.

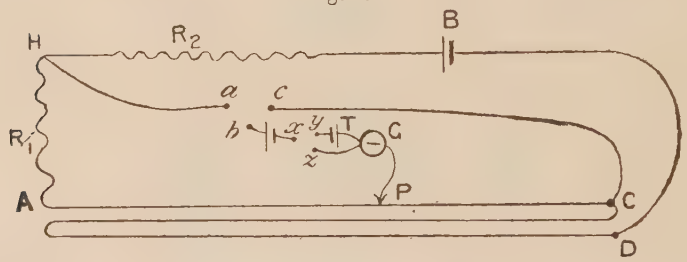
Fig. 3.



In the inner vessel containing the cells (fig. 3) the oil was circulated by means of two sets of vanes ; the upper set, A, in the form of a screw-thread, was arranged so as to send the oil downwards on rotation ; the lower set, B, were radial near the centre, and at the outer ends curved so as to force the oil upwards. Thus, as the oil was forced downwards from the first set A, it was caught, and first sent outwards and then upwards by the second set B, thus producing a constant circulation. During our experiments the temperature in this bath was always uniform to at least 0°·02 C.

The stretched wire (fig. 4), three metres in length, was employed as the potentiometer-wire. The resistance of this being known, the strength of current necessary to give a drop of potential of one ten-thousandth of a volt per centimetre length was calculated, and also the resistance required to be traversed by this current to produce a potential-difference between its extremities equal to the E.M.F. of our standard. The resistance R_1 was adjusted so as to be as nearly as possible equal to that value, and the deficit was found in terms of a length AP of the stretched wire. For example, suppose the resistance necessary to produce a potential-difference equal to the E.M.F. of our standard to be found by calculation to be 214·30 ohms. Plugs would be

Fig. 4.



taken out of R_1 to the value of 214 ohms, and the point P on the wire would be selected such that the resistance of AP equalled 0·30 ohm. Then the mercury cups a, b and x, z being joined, the current sent through the wire by the storage-cell B was adjusted by means of the auxiliary variable resistance R_2 until the E.M.F. of the standard equalled the potential-difference between the points H and P. In this way the wire was rendered direct reading for any subsequent comparisons, one centimetre on the scale corresponding with one ten-thousandth of a volt. By joining the mercury cups a, b and x, z, the value of the current could be readily checked at any time during the experiment without interrupting the rest of the circuit. As a rule, very little variation was found to occur. In this method we measure the difference of the E.M.Fs. of the cells on the stretched wire, and not the actual E.M.F. of the cell under test; consequently considerable variations of the current have very little effect on the result.

The cell under test was placed at T, and by joining the mercury cups b, c and x, y, this could be joined up with its E.M.F. in opposition to that of the standard ; and then, by moving the contact P, we could find a point such that the potential-difference between P and C equalled the difference in E.M.F. between the cell to be tested and the standard. If the difference between the E.M.Fs. was positive, then the point P lay on one side of the point C, while if the difference was negative, it lay on the other side.

To find how the changes in E.M.F. lagged behind those of the temperature, it was found best to subject the cells to a cycle of temperature, the temperature being first raised at a uniform rate through a certain number of degrees, and then cooled again at the same uniform rate to the original temperature. Supposing no lag to exist, the curve connecting temperature and E.M.F. would be the same for both rising and falling temperatures. But if there is a lag of short duration, *e.* not cumulative, then, provided that the temperature varies at the same rate throughout, two distinct curves, the second parallel to the first for the greater part of its length, will result, and will enclose an area between them. If there is any lag of a semi-permanent character, it will be shown by the curve not returning to its original starting-point till after a considerable time.

When we proceeded to take the cells through a cycle of temperature, we first of all determined absolutely the E.M.F. of the standard, which had been kept at a constant temperature for some days in the thermostat. The current for heating the cell-bath was then turned on, the required value of the current to give a certain rate of rise of temperature having been previously determined by experiment. The current in the stretched wire was then adjusted to its proper value, and the readings of the E.M.Fs. of the cells were taken every few minutes. The temperature of the cells was observed every five minutes and a curve plotted connecting temperature and time. The heating curve between time and temperature always gave practically a straight line, without readjustment of the current. When we started to cool our bath, we turned on the iced-water circulation full for the first few minutes, to get a sharp bend in the temperature and time curve, after-

wards it was made to go much slower, and the flow required readjusting from time to time to keep the curve straight. Considering that this adjustment was done purely by the method of trial and error, the curves obtained may be regarded as highly satisfactory. They approximate so closely to straight lines that it was thought unnecessary to reproduce them. In some of the cycles we cooled first and and heated afterwards, thus reversing the cycle. At the conclusion of each experiment the E.M.F. of the standard was again tested absolutely.

The accuracy of the method was all that was desired. The thermostat would keep the temperature of the standard constant to 0°·01 C., and the temperature of the cell-bath was uniform to 0°·02 at least. The difference in the E.M.Fs. could be read on the stretched wire to one hundred-thousandth of a volt. The E.M.F. of the standard never varied more than one ten-thousandth of a volt during an experiment, and in general it probably varied much less.

Three different rates of change of temperature were tried : 1° C. in seven minutes ; 1° in fifteen minutes ; and 1° in thirty minutes. A cycle generally lasted about five or six hours. The curves are plotted for E.M.F. and temperature, and, to avoid confusion, the curves for the different cells have been separated by displacing the origin of E.M.F. differences.

It will be seen that the curve connecting E.M.F. and temperature is not the same for both the rising and the falling temperatures in a cycle. As explained above, this indicates the existence of lag. The lag of E.M.F. at a rate of change of temperature of 1° in seven minutes is about four ten-thousandths of a volt ; at a rate of change of 1° in fifteen minutes, it is about two-and-a-half ten-thousandths ; at a rate of 1° in thirty minutes, it is about one twenty-thousandth of a volt. By this lag is meant half the difference between the E.M.Fs. given by the rising and falling curves for any particular temperature. If these lags are compared with those obtained for the B.T.-type of cell, in the paper above referred to, we see that the lag in the case of the H-form of cell is less than one quarter of that in the ordinary form. (See fig. 7. N.B.—The zero-lines on this diagram are purely arbitrary ; the curves are merely for the purpose of comparison.)

The temperature at the close of a cycle was sometimes not exactly the same as at the commencement, hence the temperature-time curves are not closed. It is highly probable that if the temperature had been the same, the E.M.F. would have returned to its original value. On keeping the temperature constant at the end of the cycle, the lag passed off rapidly; at a rate of variation of temperature of 1° in fifteen minutes it disappeared in about ten minutes.

The probable reason for the superiority of the H-form over the ordinary form is that when the changes of temperature produce variations in the density of the zinc sulphate solution, the zinc amalgam, being at the bottom, is always in contact with a saturated solution, while in the B.T.-form we have the solution in contact with the mercury saturated, but that in contact with zinc will not be saturated till the whole of the paste has attained its equilibrium condition and again become saturated with zinc sulphate at the new temperature.

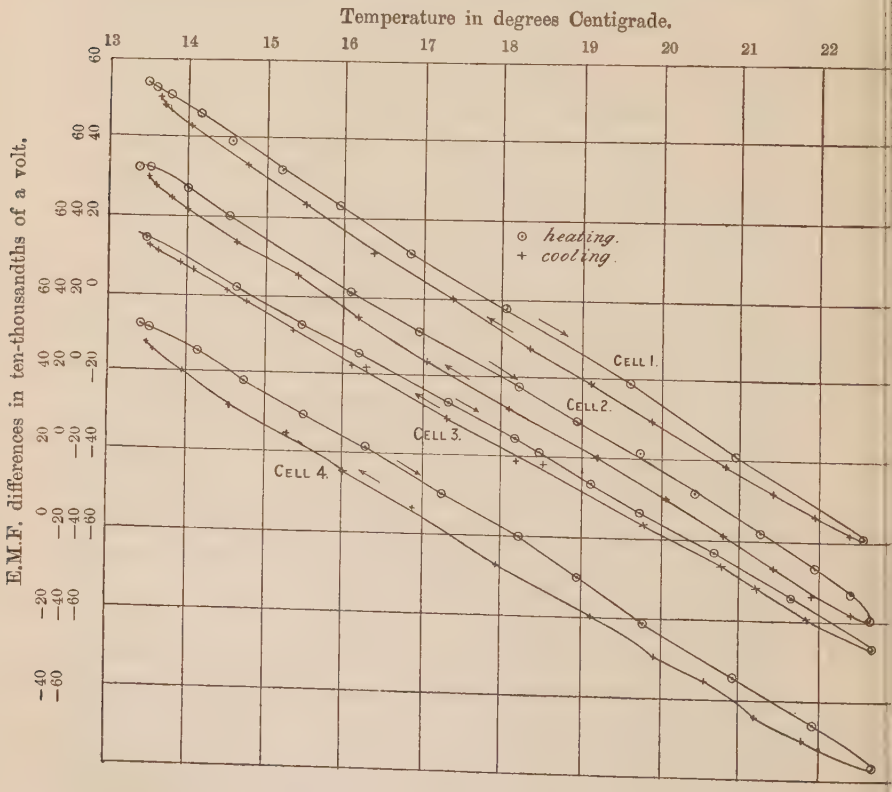
Thus in general it appears that in the H-form of Clark cell the lag is much less than in the B.T.-form, and that under ordinary conditions, when the rate of variation of temperature is not greater than 1° in half an hour, by applying the ordinary temperature-corrections we can obtain a result accurate to less than a ten-thousandth of a volt. If we wish to apply a temperature-correction for a greater rate of variation than this, we must either find the temperature-coefficient for variations at that rate from curves such as those given here, or else slightly diminish the ordinary coefficient. Subsequent experiments showed that if an H-cell were placed in a water-bath, which was heated through 3° or 4° as quickly as possible, then the E.M.F. of the cell would reach an approximately steady value as soon as the temperature of the bath, and by taking the temperature of the cell as being that of the bath, we could apply the temperature correction accurate to a ten-thousandth of a volt.

The two Muirhead cells used as standards in the above experiments were also taken through a single cycle at a rate of variation of temperature of 1° in fifteen minutes. From the curves (fig. 8) it will be seen that one of the cells is but slightly inferior as regards lag to the H-form, while the other comes about midway between the H and the B.T. forms.

These two cells have been recently dismounted, and it was found that they were almost dried up and that the zinc sulphate had crystallized out over the top of the cells. They had thus practically become examples of Prof. Callendar's crystal cell*, and this probably accounts for their small lag. The cell which appeared the drier of the two had the larger lag.

Our best thanks are due to Prof. Ayrton and Mr. T. Mather for their many valuable suggestions and kind help, and to Mr. Cooper for the loan of the cells tested.

Fig. 5.—Curves connecting E.M.F. and Temperature. Rate of Variation of Temperature 1° in 15 minutes.



* Proc. Roy. Soc. October 1897.

Fig. 6.—Curves connecting E.M.F. and Temperature; Rate of Change of Temperature 1° in 30 minutes.

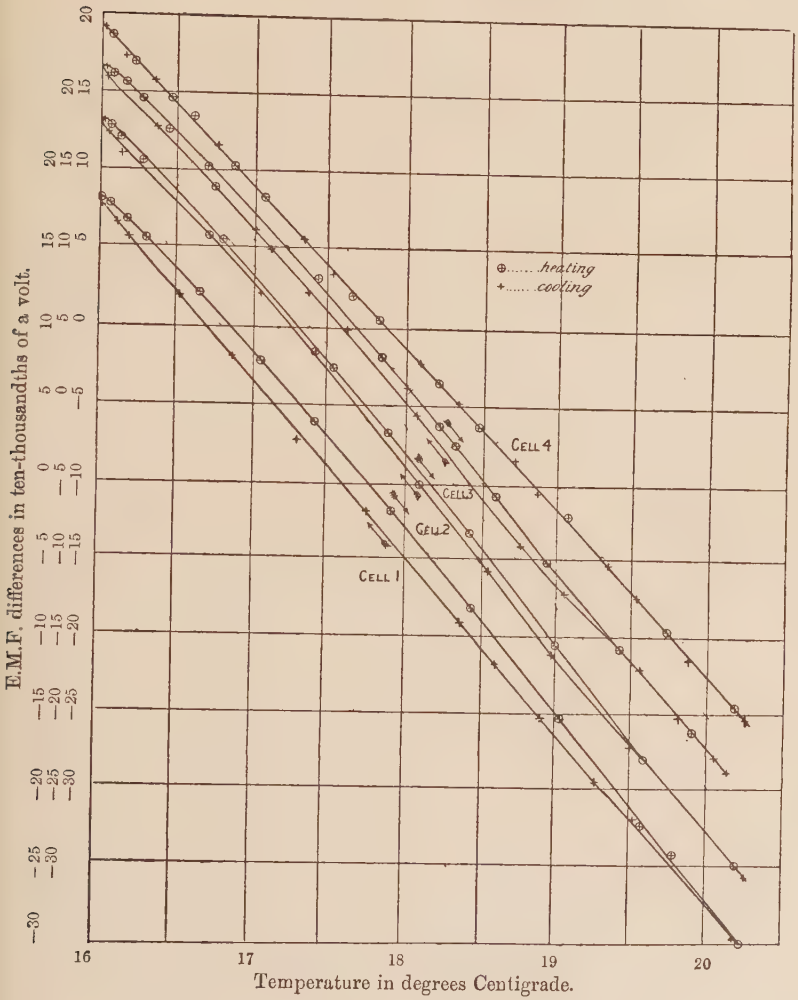


Fig. 7.—Curves comparing the lag in the H-form of cell with that in the B.T.-form.

A. An average curve for Board of Trade cell. Rate of change of temperature, 1° in 15 min. (From paper of Prof. Ayrton and Mr. Cooper.)
B. H-form. 1° in 15 min.
C. H-form. 1° in 30 min.
D. H-form. 1° in 7 min.

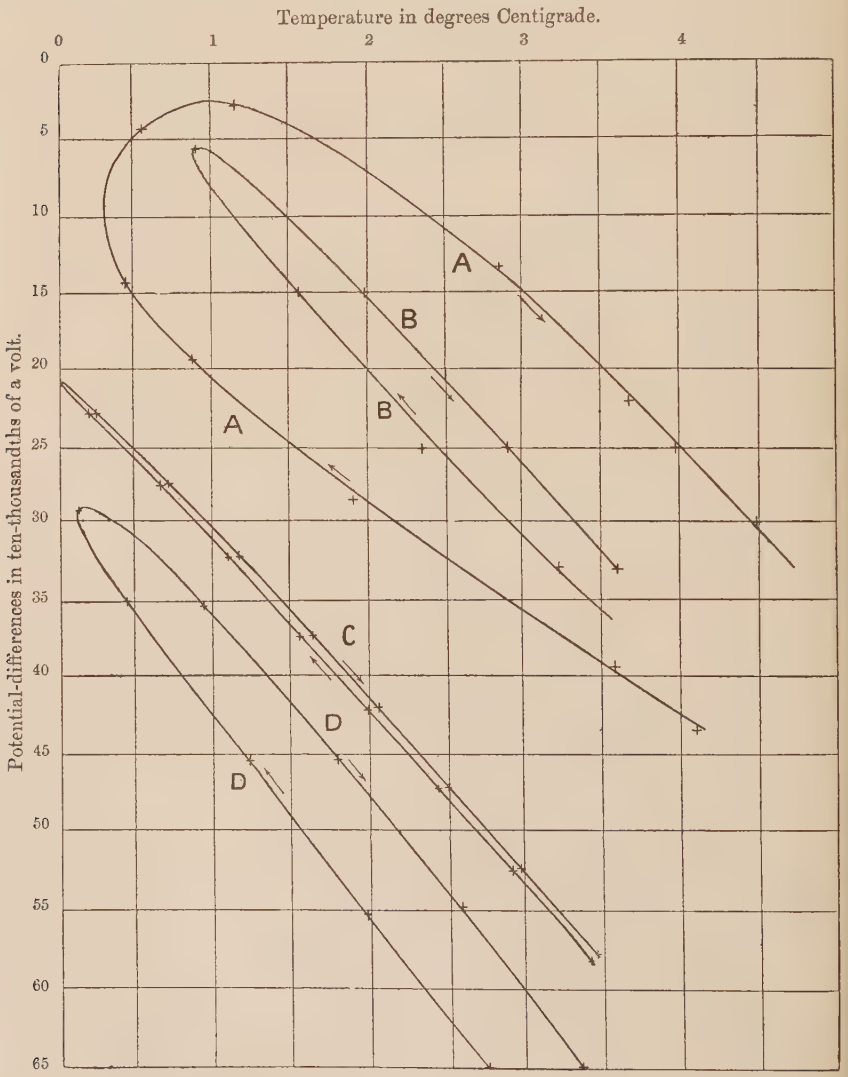
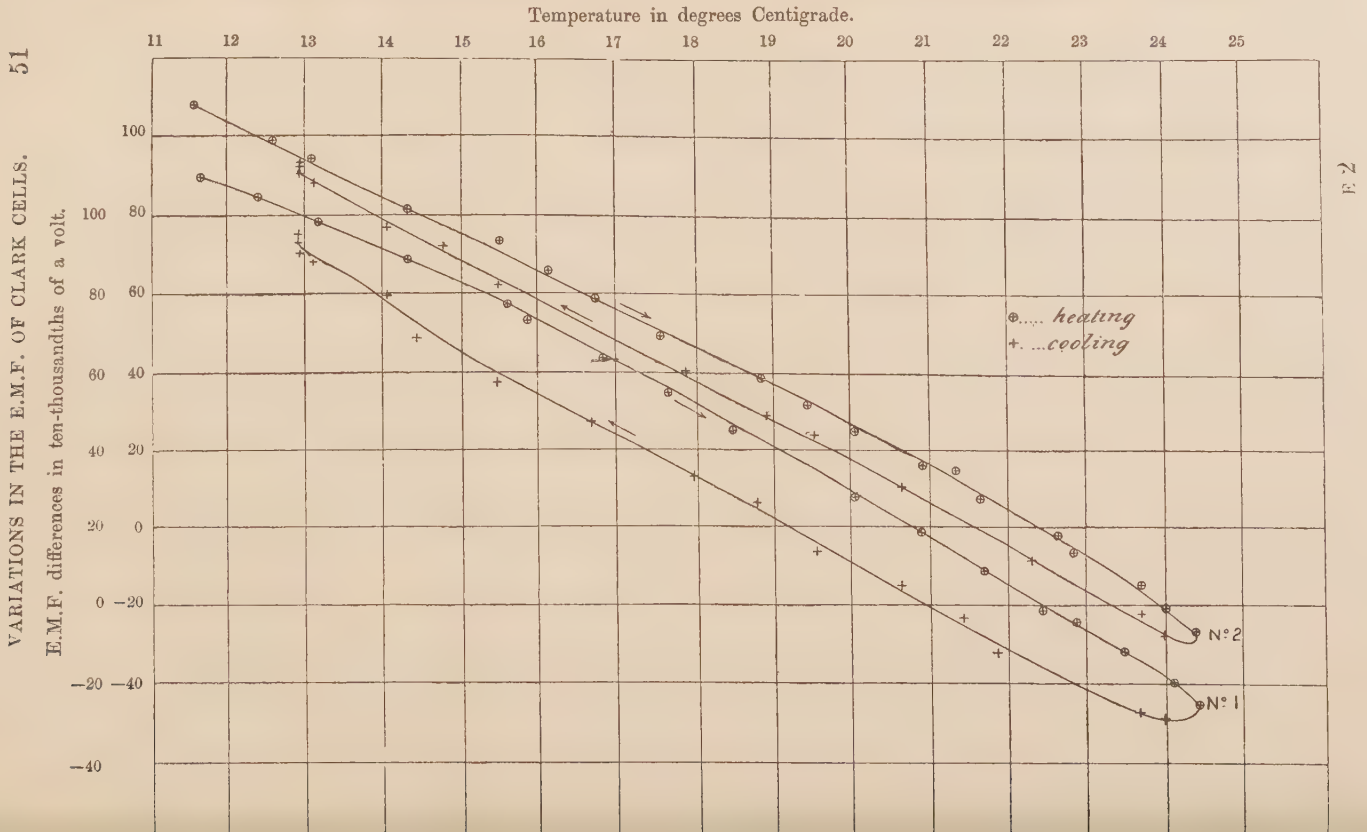


Fig. 8.—Curves of E.M.F. and Temperature. Muirhead Cells. Rate of Variation of Temperature, 1° in 15 minutes.



Appendix.

Since the above investigation was completed, some experiments have been made to test whether the lag of E.M.F. observed was due to the lag of the temperature of the cell behind that of the bath or to diffusion lag. A B.T.-form of cell was made up in a tube 2 cm. diameter, and a thermometer inserted in a position similar to that occupied by the zinc. The cell was immersed in a water-bath so that the upper surface of the paste was about 2 cm. below the surface of the water. The temperature of the bath was then raised uniformly, the bath being kept stirred, and the temperature of the cell observed from time to time and compared with that of the water-bath. The lag of the temperature of the cell behind that of the bath was, at a rate of variation of temperature of 1° in six minutes, 0°·15 ; at a rate of 1° in fifteen minutes 0°·10 ; at a rate of 1° in thirty minutes less than 0°·05. The same experiment was tried with the thermometer half in the mercury and half in the paste so as to represent the condition of things in the H-form. The temperature lags in this position were found to be very slightly less than in the other.

Experiments were also made in which the cell at a temperature of 12° C. was placed in the bath at 30° C. and the bath stirred; the temperature of the cell was observed at different times. The observations showed that at the end of five minutes the lag behind the temperature of the bath was 0°·10, at the end of eight minutes practically zero. The temperatures in these experiments were observed accurate to 0°·05 C.

A Callendar crystal cell was tested in the same manner as the above and gave almost identical results.

It appears from these experiments that about half the lag of E.M.F. observed in the H-form of cell is due to the lag of the temperature of the cell behind that of the bath, and the rest to diffusion lag.

Hence in all experiments with Clark cells, where the temperature effects are likely to be serious, it is better to discard the B.T.-form of cell and use either Prof. Callendar's crystal cell, Prof. Carhart's modification of the B.T.-form,

or the H-form. The Callendar crystal cell, in which the zinc rests in a mass of moist zinc-sulphate crystals, has a very small diffusion lag, but has the disadvantage that there is a possibility of particles of zinc falling and reaching the mercury. This fault is, however, eliminated in his inverted type. The Carhart cell, in which the solution is of constant strength, also possesses a very small diffusion lag, but it has two serious drawbacks, firstly, an error of a few tenths of a degree in the temperature at which the cell is saturated makes an error of several ten-thousandths of a volt in the E.M.F., and, secondly, every time a current passes through the cell a certain amount of zinc is dissolved, so in time the solution becomes stronger. This point is of particular importance where the cells are used with condensers.

Discussion.

Mr. W. R. Cooper thought the authors did not express the case clearly. The E.M.F. of the "Board of Trade" cell was not known to within ·1 per cent. In some cases when, for instance, cells were used differentially, greater accuracy might be required, as, for example, where a constant source of E.M.F. was being compared with the variations of another source. He would like to know with what degree of accuracy the E.M.F. of the standard cell was determined by the authors. The lag that occurred in the "Board of Trade" cell was probably due to diffusion, crystallization, and solution.

Mr. Waters said that the E.M.F. of the standard was measured by a Kelvin balance to one in ten-thousand.

IX. *A Mechanical Cause of Homogeneity of Structure and Symmetry Geometrically Investigated; with special application to Crystals and to Chemical Combination.* By William Barlow.*

[Abstract only. Published *in extenso* in *Scientific Proceedings of the Royal Dublin Society*, vol. viii. (n. s.) part vi., no. 62; also in *Groth's Zeitschrift für Krystallographie und Mineralogie*, xxix. 433, 1898.]

This paper is to be regarded as supplemental to the geometrical work on the nature of homogeneity by the same author, by which he has shown that every homogeneous structure, whatever its nature, displays one or other of the thirty-two kinds of crystal symmetry †.

The author begins by alluding to the various artificial devices for packing together a number of similar bodies which have from time to time been employed to imitate the different kinds of symmetry displayed by crystals; also to the evidence afforded by stereo-chemical investigations that a regular repetition in space which portrays the homogeneity of structure of crystals should be that of groups composed of two or more individuals rather than that of single bodies, as generally hitherto represented. He then remarks that probably the simplest conceivable kind of closest packing which gives diversity in unity of the elements of the structure, is that of a large number of spheres of two, three, or more different sizes, and argues that the condition of closest packing leads to homogeneity in a great number of such cases.

In order to avoid arbitrariness he suggests the use of elastic deformable balls, differing in material as well as in size, instead of that of rigid undeformable ones; and he points out that if an assemblage of this kind is subjected to some uniform compression which flattens the balls at the places of contact, it can be regarded as equivalent to a flock of mutually-repellent particles occupying the places of the ball-centres, and in equilibrium; provided that repulsion subsists only between near particles whose interaction is represented by the effect on one another of balls which touch.

* Read March 12, 1897.

† See Mineralogical Magazine, vol. xi. p. 119.

For the purpose of imitating two of the principal universal properties of molecular matter, the balls employed to form the assemblages referred to are (1) regarded as suffering contraction or expansion under change of conditions, those of one size, composed of one material, changing at a different rate from those of another material; and (2) it is postulated that ball can be attached to ball by an almost inextensible tie reaching from centre to centre through the place of contact, so that when tied in this way, the centres of two balls cannot get farther and farther apart under change of conditions, and thus the balls have, so to speak, to interpenetrate between tied centres if they expand.

A considerable number of closest-packed arrangements of balls of one, two, or more different sizes, some of which consist of groups of linked balls, and some of undetached ones, are described, and in each case the type of homogeneous structure which an assemblage presents is indicated, and it is further stated to which of the thirty-two systems of crystal symmetry it belongs.

From these examples it is contended that the following proposition is established :—

That assemblages belonging to all of the thirty-two classes of crystal symmetry result from closest-packing of balls of different sizes, when the relations between the different radii take the widest possible range of variety, and cases of packing together of spheres formed into groups in the way premised are included, as well as the cases in which the spheres are unlinked.

Facts with regard to crystal growth are then cited to show that symmetrical arrangement precedes solidification and that the accretion which takes place in cases of growing crystals is consistent with a conception of closest-packing brought about in the way described.

It is next pointed out that under some conditions the necessity for closest-packing will involve the production of *thin* curved assemblages, and the facts concerning thin curved and branched microscopic and other crystals are compared with cases of curved assemblages originated in this way.

The author then proceeds to describe the effect, in various different assemblages, of changing the conditions to which the balls are subjected past a critical point, and the con-

sequent production of dimorphous assemblages, and compares these results with a number of cases of dimorphism and trimorphism occurring in crystals.

In closing the first section of the paper, it is suggested that a *plane of readiest cleavage* is not necessarily a plane the ties crossing which are ties of minimum strength under any given constant conditions, but a plane on the opposite sides of which contiguous parts which face one another are most diverse.

The second section is devoted to the symmetrical partitioning of the artificial assemblages of balls described in the first section, and to the comparison of the properties of the similar groups or units thus obtained with the experimentally ascertained properties of the molecules of the stereo-chemist, considerable space being given to the discussion of enantiomorphously-similar groupings.

Tables of all the types of grouping possible are added, also of the number of types obtainable by twofold substitution in these typical groupings.

In the third section the author explains the formation of twinned assemblages of balls consistently with closest-packing, and compares the results with the actual crystal twins ; the properties of many of the modified artificial assemblages described closely resemble many optical and other anomalies of crystals.

Isomorphism, crystalloid structure, and some kinds of diffusion are also dealt with in this section.

In the sections 5, 6, and 7 certain results of closest-packing which resemble some of the incidents of *chemical change* are referred to.

The paper concludes with an Appendix in which the ideas that form the basis of the investigation are stated as definite concepts, and a fundamental *law of closest-packing* is enunciated.

An index is added.

Discussion.

Prof. Herschel said he was particularly pleased with the models. He thought it probable that a wide application would be found for the author's results. There was no doubt much to be learnt from models built up of spheres of two

or more sizes, but it would be necessary to learn a great deal more about these symmetrical arrangements before they could be applied with any degree of certainty.

Mr. FLETCHER said it was impossible to criticise the paper without long and careful study. From certain hypotheses the author had deduced a law of "closest packing" that seemed adequate to explain many results observed by chemists and crystallographers; at the same time admitting that the law might be presumed from other reasoning. By his models he had tried to present a picture not of the forms of atoms or molecules, but merely analogical representations of the probable structure of particles. Hitherto the research had been confined to determining the possible arrangements of particles all of one kind, but here were examples of packed spheres of various sizes. It was not quite clear how, in an elementary substance, there could be such a structure, although there certainly are cases of polymorphism awaiting explanation, as, for instance, with sulphur. The paper, with its 188 pages of MS., represented a vast amount of clear thinking and many years of admirable work.

Prof. ADAMS called the attention of Fellows of the Physical Society to the Museum at King's College, where were the original models as made and used by the early investigators of this branch of Physics.

Prof. MIERS (communicated too late for reading). The principle of "close packing" was not new, but Mr. Barlow was the first to extend it to explain solution, diffusion, and stereo-chemical problems. His remarks on the growth of curved crystals, vicinal faces, and pseudosymmetrical crystals were open to criticism. With regard to vicinal faces, however, leucite seemed to be a mineral in accord with his hypothesis. The author regarded a crystal as consisting of mutually repellent particles of different sorts; this seemed a very right way of attacking the problem of crystal structure, and would explain some recent observations of Rinne on crystals consisting of water particles and silicate particles. Further, Mr. Barlow had considered the way in which an assemblage might be broken up by the loosening of the ties, and the change of partners, among individual members. That is to say, he had considered crystallization and solution,

features quite ignored by ordinary theories. His view of crystal structure failed to explain why crystals should have faces, and gave no hint as to the controlling forces which keep mutually repellent particles together. Nevertheless it suggested, among other striking analogies, those bearing on the relationship between crystal structure and chemical constitution, and the irregularities of crystals. Mr. Barlow had opened up a very promising line of inquiry.

Mr. Barlow, in replying, said he greatly appreciated the interest shown in his work.

X. *Electrical Signalling without Connecting-Wires.* By Prof. Oliver Lodge, *D.Sc., F.R.S., Professor of Physics at University College, Liverpool.**

[Abstract.]

From the nature of the oscillatory disturbances emanating from any of the customary forms of Hertz vibrator, syntony has hitherto been only very partially available as a means for discriminating between receivers. There is, in fact, so rapid a decrease in the amplitude of the vibrations that almost any receiver can respond to some extent. Discrimination by syntony is possible with magnetic systems of space telegraphy, where the magnetic energy much exceeds the electric, *i. e.*, as between two separated inductive coils ; and by the use of such coils, appropriately applied, the author has been able to attain fair syntony even with true Hertz waves—*i. e.*, he has constructed spark-gap oscillators, with sufficient persistence of vibration, and syntonised resonators. The "coherer" principle can be applied to either a purely magnetic or to the Hertzian system. It was first used by the author in devising lightning-guards, and afterwards in his magnetic system of telegraphy by inductive circuits, each in series with a Leyden jar ; a pair of knobs in near contact, or other over-flow gap, being provided in the receiving apparatus. This was the first meaning of a "coherer" in the electrical sense as used by the author : it referred to a

* Read January 21, 1898.

single contact between two metal knobs. The term has since been extended by others to the filings-tube of M. Branly, and some confusion has arisen, for M. Branly does not consider that simple coherence and break explains fully the behaviour of his instrument. The author is disposed to agree, for he finds that the resistance of almost any form of coherer varies in rough proportion to the received impulses, and that there are other peculiarities (to be mentioned later); he is therefore inclined to think that the action cannot after all be entirely explained as due to mere "welding," but that there is something more to be learnt about it. The sensitiveness of a coherer depends upon the number of loose contacts: it is a maximum for a single contact, *i. e.*, for a needle-point lightly touching a steel spring. With this sensitive coherer, hardly any "tapping-back" is required for decoherence, but it wants delicate treatment when properly adjusted, and the greatest current through it should not approach a milliampere. On the other hand, a Branly tube rather improves under rough treatment; in such a tube the author prefers to use iron filings in the best possible vacuum: brass, too, is very good, but rather less easy to manage. Aluminium is thoroughly bad, and gold, for an opposite reason, will not work—its surface is too clean. Points, or small surfaces for making contact with the filings, are better than large surfaces. The usual method of connecting the coherer across the gap of an ordinary Hertz receiver, in parallel with the telegraph instrument and battery, has the unavoidable objection that they shunt away part of the received oscillations. With the syntonic receiver which contains no gap but a closed wire coil instead, the difficulty no longer exists, for the coherer can now be in series with the detecting instrument; and, in so far as these obstruct the oscillations, they may be shunted out in various ways, as the author describes. The main feature of his new syntonised vibrators is this self-inductance coil, whose function it is to prolong the duration of the oscillations, and thereby to render syntony possible. Although such a coil acts disadvantageously in so far as it possesses resistance, the resistance does not increase so fast as the self-induction. The coil should consist of stranded copper of highest conductivity, and it should have maximum inductance for given resistance.

For similar reasons, the capacity areas should also be of highest conductivity, their dimensions should increase outwards from the spark gap, as triangles. The receiver must have no gap, it should be accurately bridged over when a transmitter is used as receiver. The limit of speed of response depends upon the telegraphic instrument. Dr. Muirhead adapted a syphon-recorder to the purpose, because it is one of the quickest responders; he arranged it so that it could be used with intermittent currents, direct. Under these intermittent impulses the syphon moves and records the fluctuations of current on the paper; or it can be arranged to tremble so that the slip is marked with dots and dashes. Constant mechanical tremor is usually employed for decoherence, but the author finds that decoherence can be brought about by electrical means, without any mechanical tremor, by connecting the coherer momentarily to a circuit less effective as a collector than that of the proper capacity areas of the syntonised receiver. The battery and galvanometer detector-circuit may be used for this purpose, the coherer being momentarily connected to it, and while so connected letting it experience an impulse from a distance. The author has designed a revolving commutator, by means of which the coherer can be rapidly changed over from the resonating circuit to the instrument circuit, and finally to the "tapping-back" apparatus. A coherer is more sensitive when thus isolated and exposed to the full influence of the received oscillations; the subsequent detection of the effect by altered connections is very convenient for laboratory measurements. A diagram of a series of plotted measurements showed that the resistance of an undisturbed filings-tube is approximately a direct function of the intensity of the received stimulus, whether successive stimuli increased or decreased in strength. This electrical process of "tapping back" is dependable for a time, but the process if long continued fatigues the tube until a mechanical shake is employed to restore it. A large apparatus, made by Dr. Muirhead for actual distant syntonic work, was exhibited, and means were shown for protecting it and isolating the coherer when its own receiving areas were being used as emitters; also a special switch which is used for changing at one movement all the connections from "sending" to "receiving."

Discussion.

Prof. Threlfall said he had come to the same conclusion as Prof. Lodge as to the advisability of diminishing the number of contact points in the coherer. He had endeavoured to produce longer and more persistent waves, and thus to set afield greater effective energy. It was desirable to keep the waves as parallel as possible. He thought there was some probability that the wave-fronts could be altered and rendered more conformable by a process of diffraction.

Mr. Rutherford also had found it best to work with long waves. He fully appreciated the advantage of increasing the capacity of the oscillator by extending the surface of the metallic plates.

Mr. Campbell-Swinton asked whether experiments had been made to verify Hertz's results as to the influence of reflectors behind oscillators and receivers. He had found them disadvantageous. A single wire behind either apparatus seemed partially to annul the effect. He also asked whether Prof. Lodge had observed the extraordinary sensitiveness of coherers to small changes of current in neighbouring circuits.

Prof. Lodge, in reply, said he had observed the sensitiveness to slight sudden variations of current referred to by Mr. Campbell-Swinton; for instance, when electric lamps were switched on and off. The effect of mirrors had been studied by Prof. Fitzgerald. They required to be of large dimensions as compared to the oscillator and receiver, otherwise the true reflections were not obtained.

XI. *Exhibition of a Tesla Coil.* *By* Prof. Silvanus Thompson, *D.Sc.*, *F.R.S.**

This apparatus is intended to replace the two induction-coils and spark-gap arrangements used by Mr. Tesla for high-frequency experiments. It consists of an induction-coil, with a separate self-inductance coil in the primary circuit. This self-inductance coil is also used as an electromagnet for the separate interrupter of the primary circuit. A condenser of 2·27 microfarads capacity is connected between one end of

* January 21, 1898.

the primary coil and one terminal of the interrupter, so as to include both of them between its terminals. The primary consists of 5 turns of copper strip. The secondary has 2000 turns of thick wire. The supply current, about half an ampere, may be taken from the electric-light mains at almost any voltage from 50 to 200, direct or alternating.

Discussion.

Prof. Lodge said that he believed Sir W. Crookes had found it work quite well at 10 volts. He pointed out also that if the straight discharge rods at the spark-gap were free to slide, the discharge drove them back into their sockets.

Prof. Fitzgerald said it was stated at Toronto that the spark was broken at the interrupter when the condenser was charged, and that by the time the condenser was ready to discharge, the contact at the interrupter had been made again. It had seemed to him at first that the condenser discharges and surgings must take place at a rate far higher than the period of the mechanical movement of the interrupter; but he perceived afterwards that it was not so.

Prof. Herschel asked if such an apparatus was suitable for work with Röntgen rays.

Dr. Thompson, in reply, congratulated Mr. Tesla upon the perfect working and compactness of his invention. The present form was not suited for Röntgen ray experiments, with ordinary focus-tubes, but Mr. Tesla had designed a special tube that was excellent for that purpose.

XII. *Winter Observations on the Shadow of the Peak of Tenerife, with a new Method for measuring approximately the Diameter of the Earth.* *By* Rev. T. C. Porter, *M.A.**

[Abstract.]

The method consists in observing the shadow cast by the Peak upon the sea, and measuring the time that elapses between the moment when the apex of the shadow touches

* Read February 26, 1898.

the sea-horizon, and the instant when it is eclipsed by the shadow of night.

The author calls attention to a phenomenon hitherto unnoticed, *i. e.*, that the heated air ascending from the Peak casts a shadow, seen as a faint prolongation of that of the Peak, rising obliquely from its apex. A photograph was exhibited, taken on a quarter-plate, in which is visible the curvature of the horizon as viewed from the altitude of the Peak.

An interesting series of photographs, illustrating the conformation of the Peak and the phenomena of sunrise and twilight in that latitude, was also shown. In regard to twilight, it is noticed that the first approach of night, as observed looking eastward, is marked by a dark border of about five degrees width, followed by a sky somewhat lighter.

XIII. *A new Theory of Geysers.*

By Rev. T. C. Porter, *M.A.**

[Abstract.]

The theories of Bunsen and others fail to explain why the geyser throat appears almost completely full at the end of an eruption. This immediate re-filling is the more remarkable when it is remembered that some of the geysers of the Yellowstone region discharge a million and a half gallons at each eruption, and that the eruptions may occur at five-minute intervals. Moreover, the theories generally accepted assume steeper temperature-gradients than those in a region like Yellowstone. The author suggests that the phenomena are better explained on the assumption of an arrangement of strata such as exists in artesian-well districts; the throat or shaft of the geyser being in the position of a well communicating with a subterranean stream—the "tube" of the geyser. From the disturbed nature of the region, the tube of the geyser probably follows a waved course. The "shaft"

* Read February 26, 1898.

rises from the crest of the terminal wave; the other crests may act as steam-traps. Since a basin-like formation is characteristic of all geyser regions, it is fair to assume that the end of the tube remote from the shaft has an outcrop in the hills that form the sides of the basin. By means of this outcrop, water continually flows into the tube. Where the tube does not sink deeply enough to attain the temperature necessary for the generation of steam, a quietly-flowing hot spring is the result. But if at any point the tube descends to underground temperatures sufficiently great, steam is formed, and is trapped at the highest point of a bend. Ultimately this steam checks the flow of water, until the accumulated head of cool water from the hills overcomes the resistance, condenses the steam, and re-establishes liquid continuity. Urged by the pressure behind it, the steam is impelled towards the geyser throat; it forces the hot water before it until equilibrium is once again restored in the tube.

XIV. *A Method of Viewing Lantern Projections in Stereoscopic Relief.* *By* Rev. T. C. Porter, *M.A.**

[Abstract.]

A slotted disc rotates in front of two lanterns. These project two stereoscopic views in rapid alternation upon a screen in such a way that the two projections are approximately superposed. In the rim of the disc other slots are cut, through which the observer looks. The arrangement of slots is such that the right or left eye is only able to see the screen at the moment when its own picture, *i. e.*, the picture from the right or left lantern, is on the screen. When the rotation is sufficiently rapid, the views appear as one, without "flicker," in stereoscopic relief.

* Read February 26, 1898.

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